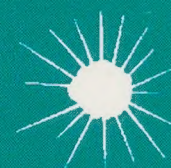


The Bulletin of Zoological Nomenclature

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International Commission
on Zoological Nomenclature



ICZN

THE BULLETIN OF ZOOLOGICAL NOMENCLATURE

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Cover image: *Portunus pelagicus* (L.), the blue swimming crab native to Indo-Pacific waters, Plate 49 from *Zoological drawings* by F.L. Bauer (© The Natural History Museum, London, 1997, housed in the collection of the General and Zoology Library).

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Notices

(1) Applications and correspondence relating to applications to the Commission should be sent to the Executive Secretary at the address given on the inside of the front cover and on the Commission website. English is the official language of the *Bulletin*. Please take careful note of instructions to authors (present in a one or two page form in each volume) as incorrectly formatted applications will be returned to authors for revision. The Commission's Secretariat will answer general nomenclatural (as opposed to purely taxonomic) enquiries and assist with the formulation of applications and, far as it can, check the main nomenclatural references in applications. Correspondence should be sent by e-mail to 'iczn@nhm.ac.uk' where possible.

(2) The Commission votes on applications eight months after they have been published, although this period is normally extended to enable comments to be submitted. Comments for publication relating to applications (either in support or against, or offering alternative solutions) should be submitted as soon as possible. Comments may be edited.

(3) Requests for help and advice on the Code can be made direct to the Commission and other interested parties via the Internet. Membership of the Commission's Discussion List is free of charge. You can subscribe and find out more about the list at <http://list.afriherp.org/mailman/listinfo/iczn-list>.

(4) The Commission also welcomes the submission of general-interest articles on nomenclatural themes or nomenclatural notes on particular issues. These may deal with taxonomy, but should be mainly nomenclatural in content. Articles and notes should be sent to the Executive Secretary.

New applications to the Commission

The following new applications have been received since the last issue of the *Bulletin* (volume 65, part 4, 20 December 2008) went to press. Under Article 82 of the Code, the existing usage of names in the applications is to be maintained until the Commission's rulings on the applications (the Opinions) have been published.

CASE 3483: *Villanyia exilis* Kretzoi, 1956 (Mammalia, Rodentia, ARVICOLIDAE): Request to set aside taxonomic acts subsequent to the original description until the lectotype designation of *Terzea* (1991) in order to conserve prevailing usage. D.F. Mayhew.

CASE 3484: NOMIIDAE Gozis, 1875 (Insecta, Coleoptera): proposed emendation of spelling to NOMIUSIDAE to remove homonymy with NOMIINAE Robertson, 1904 (Insecta, Hymenoptera). M.S. Engel & P. Bouchard.

CASE 3485: *Lychnorhiza lucerna* Haeckel, 1880 (Cnidaria, Scyphozoa, Rhizostomeae): proposed conservation of generic and specific names. A.C. Morandini.

CASE 3486: *Betta anabatooides* Bleeker, 1851 (Osteichthyes, Perciformes): proposed designation of a neotype. T.H. Hui & P.K.L. Ng.

CASE 3487: *Megalosaurus crenatissimus* Depéret, 1896 (currently *Majungasaurus crenatissimus*; Dinosauria, Theropoda): proposed replacement of the holotype by a neotype. M.T. Carrano, D.W. Krause, P.M. O'Connor & S.D. Sampson.

CASE 3488: *Papilio danae* Fabricius, 1775 (currently *Colotis danae*; Insecta, Lepidoptera, PIERIDAE): proposed conservation of prevailing usage by the suppression of *Papilio danae* Hufnagel, 1766. T.B. Larsen & R.I. Vane-Wright.

The International Commission on Zoological Nomenclature

The aim of the Commission is to bring stability to the use of animal names (zoological nomenclature). The Commission does this by: (a) producing, publishing and periodically revising the *International Code of Zoological Nomenclature* (the Code), which deals with the formulation and use of animal names, and (b) considering and ruling on specific cases of nomenclatural uncertainty and dispute about animal names that are not automatically resolved under the provisions of the Code, via applications published in the *Bulletin of Zoological Nomenclature*. Opinions are official rulings of the Commission and come into effect on the day of publication of the *Bulletin*. The Opinions are summarised in the *Official Lists and Indexes of Names and Works in Zoology*. The *Bulletin* also includes papers on general nomenclatural issues and proposed amendments to the Code.

The International Congress of Zoology founded the Commission in 1895. At present, the Commission consists of 22 zoologists from 18 countries, with six more to be elected shortly, whose interests cover most of the main aspects of the animal kingdom, including palaeontology. The Commission is under the patronage of the International Union of Biological Sciences (IUBS). Commission members are elected by the vote of zoologists attending General Assemblies of the IUBS or other appropriate congresses. Nominations for membership may be sent to the Executive Secretary at any time. Further discussion of the Commission's activities can be found in BZN 48: 295–299 (December 1991), BZN 60: supplement, pp. 1–12 (March 2003) and details of the Commission's publications are given on the inside back cover herein and on the ICZN website (www.iczn.org).

Members of the Commission are listed on the inside front cover herein.

The International Trust for Zoological Nomenclature

The International Trust for Zoological Nomenclature (the Trust) was founded in 1947 to manage the Commission's financial matters. It is a registered charity, based in the U.K. (No. 211944). At present, the Trust consists of 31 members from 15 countries. Discussion of the Trust's activities can be found in BZN 60: supplement, pp. 1–12 (March 2003) and on the ICZN website.

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The International Code of Zoological Nomenclature

The aim of the Code is to provide the greatest universality and continuity in the scientific names of animals without restricting the taxonomy or classification of the animals for which the names are used. The current (fourth edition) of the Code was published by the Trust in English and French in 1999, and came into effect on 1 January 2000; this edition supersedes all previous editions. Official texts are available in Catalanian, Chinese, Czech, German, Japanese, Russian, Spanish and Ukrainian. See the Commission's website (www.iczn.org) for ordering details.

Contributions to the Discussion on Electronic Publication

Introduction

The ICZN drafted a proposed amendment to the Code that would allow publication of nomenclatural acts in exclusively electronic media to be valid and available (published in BZN 65: 265–275, several other sources, and available here http://www.iczn.org/electronic_publication.html). This proposed document is open for input, and will receive the first consideration at the IUBS meeting in early November 2009. If it passes IUBS consideration, the document will be fine-tuned and subsequently voted on by the Commission. Details of this process are provided in the text accompanying the proposed amendment.

There has been lively discussion of this proposed amendment and the various issues it raises for nomenclature and taxonomy in several online listservers. We have also received comments contributed directly to our office. To bring the comments together by topic, to reach a different audience and to avoid the irony of losing the basis for the final decision because it was not published on durable media, we have assembled a selection of comments here. We feel there is great utility in having these assembled. As for any good debate, these comments may be impassioned, contradictory, insightful, or missing the mark. Yet they allow us to take the measure of the needs and perceptions of the taxonomic community for the future implementation of nomenclature in an age of digital communication.

Comments have occasionally been edited for consistency, readability and style. We attempted to preserve the author's (perceived) original intent, and apologise for any inadvertent changes. The sources for these comments are largely the ICZN listserver (<http://list.afriherp.org/mailman/listinfo/iczn-list>) and Taxacom (<http://mailman.nhm.ku.edu/mailman/listinfo/taxacom>). Further input is welcome, either to the listservers or directly to the ICZN secretariat.

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General comments

I have seen many 'red herrings' about e-publication. As far as I am concerned, the three most important 'real' issues are as follows: 1) Paper publications have a *much* longer history/track-record than electronic archiving. 2) Electronic files cannot be read directly by humans; they minimally require (at least with today's technology) three things: appropriate hardware, appropriate software, and electricity. 3) Compared with paper documents, electronic files are *much* cheaper and faster to replicate

and distribute globally in large quantities, and it is *much* easier to assure that copies are identical. So, as should be obvious, 1) & 2) favour paper, and work against electronic documents; 3) favours electronic documents. [...] The e-publication paradigm does not need to be (indeed, *can not* be) perfect – it need only be as good as, or better than, paper-based publishing. **R. Pyle**

I believe electronic publication is inevitable and moreover that it will enhance the effectiveness and impact of taxonomy. The challenge I am wrestling with is how not to lose the functionality and robustness we have achieved since Linnaeus in the process of changing media. In taxonomy, preserving the chain of evidence on information through time is absolutely critical – and the unit of time is the century. **J. Croft**

Publication quality – Risks and benefits

There is concern that by allowing electronic publication to be Code-compliant, we are at the same time allowing or even encouraging ‘sub-standard’ taxonomy to flourish. I share this concern, and although such ‘sub-standard’ taxonomy is in no way prevented or even constrained by the present Code, the ease and speed of producing an electronic publication (however this is defined) could be feared to cause an explosive growth in ‘sub-standard’ e-only taxonomy. But is this fear warranted? Could we perhaps at least partly prevent this, either through the requirements of proper archiving, which necessarily must be put in place, or by making registration in ZooBank a requirement for e-only published names and nomenclatural acts? **T. Pape**

BioMed Central publishes open access, peer reviewed articles in all areas of biomedical research. The journals of the BMC series (e.g. *BMC Biology*, *BMC Evolutionary Biology* and *BMC Ecology*) are supported by outstanding Editorial Board members and both the Editorial Board and the in-house staff take care that peer review is performed at the highest standard. Once published, the formatted PDF and HTML versions of an article are final and we do not allow authors to make post-publication changes. **H. Zauner**

Why is Recommendation 8D on immutability of the publication not mandatory and part of the Code itself?

Furthermore, Recommendation 8C encourages those who publish electronically to also publish on paper. This raised a question about having two versions of the same entity, and which would be ‘the’ published version. A cross-reference to article 21.9 would help clarify the situation. It still leaves open the situation that an electronic and paper publication may have differing formats (PDF could make both look the same, but other solutions wouldn’t), so I wonder if there would be any issues with having two versions of the one publication? **M. Jackson**

Publication quality – Should peer review be part of the discussion?

The Commission has neither the mandate nor the resources nor the intent to assess quality. A name or a nomenclatural act may be available, or it may not. Taxonomy may be ‘good’ or ‘bad’ depending on the outcome of any given review. The Code does give very precise requirements for when names are ‘legal’, i.e. Code-compliant, and

therefore available. Scientific quality remains (and should remain) outside the Code and in the hands of authors, editors, peers, assessment committees and others. Code-compliance is probably to some extent uncontroversial as this is regulated by the Code, and if/when ZooBank is up and running we will get a much better handle on all newly proposed names. ZooBank probably will provide at least some degree of peer review for Code-compliance, as it requires input of publication and type information. **T. Pape**

I think it would be reasonable to include a requirement for names to have peer review before they are legal. This does not stop someone from finding a few friends to provide sympathetic review, or even falsely claiming to have gotten review, but it would provide a constraint. The present wording seems to allow me to simply put a number of new taxa into a file online as long as I send the names into the official registration, format and distribute the file appropriately, and follow the regulations about type designations, etc. Given all the stuff that goes online, there seems to also be a very real risk of inadvertent publication of names. Having the requirement of peer review would be another control; someone who is just putting information on the web would be unlikely to accidentally insert a statement about it being peer reviewed for taxonomic purposes. Standard journals already carry statements about the peer review process, so this would seem to only affect books and more irregular publications (though there are many of the latter in malacology).

D. Campbell

The lack of peer review is not specifically a problem for web-based publications as there are a few 'basement publications' around already. It is true that editors of such publications could bypass a demand for review by asking friends or relatives to be reviewers. A radical solution to this problem might be to demand that new names and nomenclatural changes are only published in publications that are supported by scientific institutions and societies (this support could simply be that the editor in chief is employed by an institution or appointed by a society). It may be impractical, but is it more impractical than the problems caused by names and nomenclatural changes published in 'basement publications'? **T. Simonsen**

I am Editor-in-Chief of *Parasites & Vectors*, an open access, online journal, published by BioMed Central. Online journals offer the same degree of permanency and quality as journals produced in hard copy, and this should qualify them to publish the results of taxonomic research that will be recognised by the ICZN. *Parasites & Vectors* has an Advisory Board and Editorial Board comprising international figures of importance in vector biology and parasitology, thus the quality is of equal standing to print journals. I believe that my comments apply to all BMC journals and to those of other major online publishers, such as PLoS. **C. Arme**

Reviews are suggestions of referees to the author. The author decides whether s/he wants to accept them. The editor decides whether s/he agrees with the author's decision and consequently accepts or rejects the paper. If we give referees ultimate power or try to achieve a consensus before publication, then we will probably avoid some unethical behavior, but we will only publish mainstream work and significantly slow down progress in science. **F. Krell**

In the traditional system, descriptions of new taxa are published in printed journals and many reviewers may check the validity of the new taxa. Thus, a good quality control system remains intact which is not available to many website publications. This is not a mature time for the ICZN to pass this amendment and so it should be rejected. I am sure that you will receive opposing comments like this from taxonomists from all over the world. **V. Prasad**

[**N. Evenhuis** said] ‘Science, by definition, allows for the repeatability of hypotheses (i.e., testing hypotheses). In taxonomy, those hypotheses are one person’s ideas about what constitutes a taxon including how that taxon is defined and circumscribed’, which is one of the reasons I am so disgusted with what we presently accept in the way of peer review. Many folks have interpreted my stance as tweaking the Code so it subsumes peer review (and therefore is equated with censorship), when what I am ‘functionally’ aiming for is to tweak ‘peer review’ so that it will be compatible with the aims of the Code: namely, stability of nomenclature. If names are published that *everyone knows* are homonyms, synonyms, unjustified emendations, inappropriate lectotypes, or whatever, then they create nomenclatural instability by their very existence in print; the core of the problem is that those works were published in the first place. I am not advocating censorship over matters dealing strictly with taxonomic opinions (which ‘obviously’ can vary and change), but in cases where there is clear and obvious error or actual ‘bad faith’ on the part of authors, who would normally find a way to circumvent true and proper peer review, either by accident or design. There are enough such authors throughout our discipline that the nature of the problem and its reality cannot be ignored. For every hundred honest taxonomists, there is a person with fine credentials but who knowingly violates the Code and publishes ‘anyway’. Basically, nomenclature is at the mercy of a thousand different journal editors, many of whom know little or nothing about the Code or its proper application. If the entities presently overseeing scientific publication cannot handle the responsibility (and given that self-publication is still an option, then by definition such authors cannot be stopped conventionally), and if this works against our interests (and therefore against the good of science), then to me it is worth suggesting that we either assume that responsibility or come up with a plan that the broader community can follow to better attain the goal. We need a way to prevent the frauds, crackpots, and incompetents from creating worthless ‘available names’ that does NOT infringe on the legitimate pursuit of science. My vision of community review is the best I can conceive of to meet this aim. **D. Yanega**

Existing media – CD & DVD

We take issue with parts of the proposal because we look back on a rich and positive experience with interactive electronic publications (e.g. on CD/DVD) over more than ten years. We want to continue to contribute to the rapid development of scientific taxonomic research with the innovation of adequate electronic publishing.

We suggest a rewording of *Article 8.4.* as follows: *To be considered as published, works must satisfy the following criteria: 8.4.1. Works must be published and archived on a physically ‘hard’ medium (e.g. on a CD/DVD or on paper at present).* Reasons: 1. Since 2000, the ICZN has allowed taxonomic and scientific works to be published on media other than printed paper (e.g. CD/DVD). This has led to many innovations that rank among the standards of scientific research and publishing today. 2. All

these new interactive ways of presentation cannot be documented on paper without a large loss of data and information. 3. For this reason limiting documentation of taxonomic-scientific works only to printed paper is no longer an option! 4. The core task of the ICZN and the rules is to make sure that taxonomic-scientific results remain available without loss or change independent from the storage medium. The question of the 'half-life' of a CD ROM or the 'acid content' of paper becomes subordinate, because the dynamic advancement of future data carriers has to be part of this data preservation concept. 5. The subject has never been – neither before nor after Gutenberg – the physical stability of data carriers, but the question has always been 'How can it be ensured that data can be passed quickly, comfortably, confidently, error-free and cost-efficiently to the next generation of data carriers?' Biologists should know it best: 'To preserve *life* means to copy life successfully!

We suggest that 8.4.2. should be deleted and replaced with: *Copies of 'hard' physical media have to be archived in at least 5, or even 10 or 25 . . . public libraries; these have to be mentioned by name in the work.* Reasons: 1. As the currently valid code (see Article 8.6) does not set a time in which new methods of publication are to be tested, a revision of this rule would be a serious intervention in commercial vested rights (e.g. involving development costs for interactive programmes, the costs of CD distribution). 2. The rapid scientific advancement in publishing taxonomically significant works during the past 10 years – initiated, among others, by the ICZN – has been accompanied successfully and adequately by digital interactive presentations (e.g. a very large number of pictures, scroll pictures, overlays, microphotography, and computer tomography). This innovation, which allows the extensive documentation of empirically established hypotheses to the reader, cannot be realised on paper (including digital paper, PDF) – not even approximately.

We propose the following rules for works that are published on electronic media (e.g. internet): 8.5.1. *Electronic publications (e.g. interactive publications or digital paper/PDF) must be archived and distributed within 1 year by the editor on a physically 'hard' medium.* 8.5.2. *When work is archived on different media it has to be ensured that a 1:1 reproduction (this means the format remains the same) is guaranteed.* 8.5.3. *In the case of interactive publications, such as CD/DVD at the present time, and in the case of digital paper publications (PDF) it is possible to use paper as well.* Reasons: 1. In principle we appreciate this overdue development for publications on electronic media! 2. It must be assured that the original form and content of the publication – irrespective of the choice of the first medium – are preserved (e.g. interactive publication first on CD ROM and then the interactive structure in the internet, or first publication on paper and then PDF in the internet). A coloured digital publication (PDF) for example, is incompatible with black-and-white publication on paper (information loss or deviations have to be avoided!). *Final comment and recommendation:* Why should we be at odds about this? To question the durability of the media used (e.g. CD ROM or books) is a platitude. The same is true for questioning the durability of archiving institutions. Hence, we recommend the simultaneous utilisation of numerous archiving media, institutions and official registers (e.g. ZooBank).

One of the challenges of modern library sciences is the 'digital archiving' of our archived paper publications, for which great efforts are being made internationally. Let's not go two steps forward and then three backward! **P.E. Stüben & CURCULIO Institute**

Status of e-publications before 2010

It would be useful if the amendment could clarify the retrospective status of e-only publications under the old Code. I would suggest including in the amendment a clarification/confirmation noting that e-only publications predating 2009 will be considered authoritative if they have been deposited in a printed form with a certain set of archival libraries. This would help to resolve the ambiguity in the status of articles such as in *PLoS ONE* and *BMC Evolutionary Biology* which seek to comply with the existing code as far as possible, but find it difficult to know for sure whether they have done so because aspects of the existing code in relation to electronic publication are difficult to interpret and seem somewhat self-contradictory.

M. Cockerill

The amendment is proposed to be effective from 2010. What will happen to the names published in electronic-only journals before that? It would be practical to allow inclusion of all of them from their dates of publication (perhaps this may be permitted under a special provision). **S. Singh**

The proposed amendment of article 8.5 does not take into account article 8.6 of the present code. It states that in order to be considered as published, a work issued and distributed electronically needs to have been issued after 2009. It never states the conditions for electronic publishing in the present code. In most articles of the present code, names available for nomenclatural usage that followed the rules of previous editions of the Code at the time they were published are considered valid, even if they do not follow the conditions of the present code. Article 8.6 is completely reformed in the proposal, thereby forgetting what the current 8.6 states.

In order to protect names published after 1999 under the current article 8.6, I propose the following version of article 8.5 of the amendment:

8.5. Works issued and distributed electronically. To be considered published, a work issued and distributed electronically must

8.5.1. Have been issued *after 1999*,

8.5.2. *contain a statement that copies (in the form in which it is published) have been deposited in at least 5 major publicly accessible libraries which are identified by name in the work itself, if the work was produced after 1999 and before 2010,*

Example. On 28 May 2008 Fisher and Smith published three new species of ants (Hymenoptera: Formicidae) in the open access, peer-reviewed journal PLoS ONE which is published only online. However, they deposited printed versions and PDFs in the following institutions: Natural History Museum, London; American Museum of Natural History; Smithsonian National Museum of Natural History; Muséum National d'Histoire Naturelle; Russian Academy of Sciences; and Academia Sinica. Hence their descriptions are valid.

8.5.3. state the date of publication in the work itself *if produced after 2009*, and

8.5.4. be archived in an organization other than the publisher in a manner compliant with ISO standard 14721: 2003 for an Open Archive Information System (OAIS), or the successors to that standard, *if the work was produced after 2009*. (For documentation of the location of the archive, see Article 10.9.2.1).

The remainder of Article 8.5.4 (8.5.4.1, 8.5.4.2 and 8.5.4.3) would be exactly as in the currently proposed amendment articles 8.5.3.1, 8.5.3.2 and 8.5.3.3 **E. Alvarado Reyes**

ZooBank and registration – general

Article 10.9.1 requires a registration number to be cited in ‘the work itself’, implying that before a name is electronically published it must be registered in ZooBank to obtain a number. Adding a number retrospectively would go against the spirit of *Rec. 8D*. So it’s imposing an additional burden in that an author must prepare the paper, register names (before they are published) in ZooBank, add the registration numbers to the paper, then once the work is electronically published go back to ZooBank and add full bibliographic information. Also, there is the possibility of introducing errors, say if a name was changed at the last minute. Would it still be available if there wasn’t an exact match between the registry and the publication?

Furthermore, there is the possibility of electronic works being in limbo for at least a year if they are not immediately archived. (8.5.3.3). **K. Challis**

We certainly recognise that registration of names and community review on taxonomy are two entirely different issues. Review of names should be restricted to nomenclatural points – such as homonymy, type depository, absolute synonymy based on the same specimen, quality of description (not the quality of taxonomy; for example whether the description fulfils the minimum requirement to distinguish the nominative taxon from others). If the Code governs the quality of taxonomy, the results of which are nomenclatural acts, this would mean that the Code starts governing the methodology in taxonomy, including species concepts. Evaluation of the scientific content of a work may differ according to the opinions of scientific society at a given time, but we should secure the stability of names, as they are the communication medium for any such evaluation. **J. Kojima**

It is true that we are looking backwards. It comes with the territory. Nomenclature is all about looking backwards (the Codes are retroactive), while serving the needs of the present and the future. For nomenclature, the way it works now, it is important to have an unalterable document published at a single point in time. How else to establish priority? It may be interesting to speculate about an entirely different way to go about it, with a central register as the only evidence that counts, and the ‘original publication’ only an entry in a database, but this represents more than mere evolution of what was here before. It is a complete break with the established way of doing things, and I would not even dare guess at what the consequences of such a break would be. It would mean starting all over again (like the Bacterial Code did, but even more radical, and with a lot more taxa involved), moving into practically uncharted territory. **P. van Rijckevorsel**

ZooBank and registration – Two-tiered taxonomy

There will be a problem if ZooBank registration is required of only electronically published names and changes. This will only create first and second level taxonomic works. If anything, the need for registration is lesser for the easily searchable e-publications compared to paper publications. It is very well to recommend that journal editors ask authors to register names and changes. But why not have one rule for all? **T. Simonsen**

One of the proposed advantages of registration was supposed to be there would be no surprise taxa anymore. But with this scheme there will be no wonderful database of all new species – one will still have to ferret around looking for obscure new taxa in paper publications. And without universal registration people will still be able to (irritatingly & adding to confusion) publish the same species name more than once.

G. Read

You are setting up a two-tier system, with registration being a requirement for electronically published names but not for traditionally printed names. Why should the two sorts of publication be treated differently? I can see that it's one way of ensuring that electronically published names become widely known, but surely names published in certain obscure journals are also easily overlooked. Of course, if there's some resistance to registration of names, this could be a way of introducing it gradually. However, I wonder if a partial registry would only be of limited use?

K. Challis

The sustainable business model for ZooBank/ICZN needs to be addressed in the context of e-publication. As it stands now, ZooBank is physically close to a one-man show (though a fantastic one), the ICZN Secretariat has no funding secured beyond little more than a year. Any scientific field needs to secure its infrastructure – in our case taxonomic publications and ZooBank. Sticking to print does a big disservice in a world that is electronic. Nobody will fund an organisation that is relying on finding prints from all over the world – a system that so far proved inefficient. **D. Agosti**

Increased access as a result of current technology – open access

Web-based, open-access publication is the only way forward for science (not just taxonomy). Paper-based publications (including subscription-based access to PDFs) is all very well for those of us who have access to large research libraries. But an increasing number of scientists and others with an interest in science haven't got that. Access to a computer, the internet, and electricity are often easier to come by than access to a library. And if you are physically away from the library, you need access to these three anyway. I think that *Canadian Journal of Arthropod Identification* (a web-based journal devoted to the publication of works related to Canadian Arthropod recognition and documentation) is an excellent example of the direction we could, and in my opinion should, move. **T. Simonsen**

Clearly, we need to be able to document what an author meant when s/he described a new taxon. The best way to do so is to ensure that the description is accessible online – I would, in fact, make it a requirement in the next Code that new nomenclatorial acts are valid if the underlying documents are online discoverable and available. This allows access to the baseline data we create for the whole scientific community. It opens up our science, and allows wide access to the data that is not given by most of the 1000 serials or books with taxonomic content. Such copies can easily be distributed in many archives, one of which ought to be the ICZN/ZooBank. Finally, moving from print to open-access e-publication is a very basic move to democratise our science and open it up beyond the few privileged with access.

D. Agosti

In an ideal world, access to complete articles (not just Abstracts) should be entirely open and free to all worldwide, via the internet. Copyright of all works is vested in the author but others are free to use the material, with appropriate acknowledgments. Other workers would have the opportunity to comment on publications via a facility on the home page, thereby engaging the community in debate, if appropriate.

C. Arme

Increased access as a result of current technology – Developing countries, workers without access to major libraries

Online systems are not available to many scientists in the world. In addition, scientists like to see and work with printed descriptions of taxa. **V. Prasad**

There should be provision for making the electronic publications freely available to the taxonomic workers of less developed countries. **S. Singh**

Evolutionary Biologists, as authors, embrace the advantages of online, open access publishing to ensure wide dissemination of their work and, as readers, enjoy free and easy access to research articles from anywhere in the world. We are enthusiastic about initiatives to share and cross-link taxonomic and biodiversity data and we are committed to supporting the scientific community in their task of describing the vast number of species that are still unknown to science. We are aware of the importance of stability of species names. We do not produce print issues of our journals, but we ensure that our content is permanently available to everyone. Following publication, the full text of each article is immediately and permanently archived in PubMed Central, the US National Library of Medicine's full-text repository of life science literature, and also in repositories at the University of Potsdam in Germany, at INIST in France and in e-Depot, the National Library of the Netherlands' digital archive of all electronic publications. BioMed Central is also part of the LOCKSS initiative which helps libraries to archive digital content. We are convinced that the open access, online articles published in our peer reviewed journals are at least as permanently accessible as articles in print journals, with the added benefit of wide and free dissemination and the advantage of being able to apply new tools to manage and explore biodiversity data. **H. Zauner**

Electronic-only publications should be allowed if mechanisms can be found that give reasonable assurance of the long-term accessibility of the information they contain. Thinking in terms of advancing technology, and the tangled unknowns of 'permanence' on the web and of software formats, it seems rather likely (to me) that in the year 2109 the information contained in these electronic-only publications will only be accessible via the printouts done by the scientists in the early 21st century! I'm quite sure that's not the intent of 'long-term accessibility' here though. What precisely is a 'reasonable assurance of long-term accessibility'? **S.D. Gaimari**

Speed of taxonomic work and benefits of atomized, cross-linked content

Consider the time when your virtual expedition makes use of a parataxonomist somewhere remote who collects a living organism, produces a high resolution three dimensional image from which you derive a description, sequences the entire genome which you analyse and, Eureka!, you recognise that you have an undescribed species.

In your working environment you give your reasons for its novelty and press the button marked publish (having previously registered the name with whatever 'Name Bank' is appropriate). But then, at the speed of light, your new species is instantly synonymized everywhere it is considered relevant by automated systems which compare your description with everything that is known of the parent taxon of your new species and all its nearest relatives . . . and the systems come to a different conclusion (ditto with the molecular data). That's the sort of peer review that's possible when everything is joined up. It's no different really from what happens now it's just that the synonymy is much faster.

We are still stuck thinking of a future in the two-dimensional world of digital sheets of paper instead of the highly atomized data we should be aiming for. Equally, when we think of the information from the past, we are thinking of reading digital representations of the real sheets of paper rather than thinking about working with the atomized content (with the images of the original pages in the background somewhere just in case they are needed). **P. Kirk**

Format issues and retrievability in the future – general

Documents themselves will be carried forward, but I don't think the file format issue is a red herring, or that it's a lesson that's been well-learned. The big problem is that the de facto standard is PDF, a proprietary format that can't be easily reverse-engineered to display correctly if you have the file but not Acrobat (as far as I know; maybe I'm wrong?), or even have the text extractable from a non-functional file (and of course some don't have 'text' at all, just an image of the writing). I already have PDF documents from five years ago that refuse to open. I haven't tried that hard to recover them, so maybe they could be repaired, but I'd rather not be taking that kind of risk on things like descriptions that are supposed to last at least as long as paper documents. **K. Magnacca**

There are a number of different PDF viewers available for Windows, Mac and Unix platforms. However, they don't all support all the features that Adobe stuffs into its version of PDF. If we can agree on a sufficient subset of features, it is entirely possible to produce PDF files that will be readable into the future, with or without the ongoing existence of Adobe. That said, it would also be possible for Adobe, or any other software company, to lure us into using some flashy, non-portable extension that would effectively lock us out of our documents at the whim of said company. While we haven't yet fully addressed this issue, it is possible with existing technology. I also believe it is essential, if we aren't going to find ourselves stuck with ever-expanding back-catalogues of papers that need to be laboriously translated from one format to the next. **T.W. Smith**

No one can guarantee that information contained in electronic-only systems or mechanisms will be given assurance of accessibility in terms of centuries or even of decades. Electronic-only publications should be allowed, but this should be accompanied by the system archiving the information, at least on the nomenclatural acts in paper-based printouts that will be deposited and maintained at several institutions in each region. Losing the information in taxonomic works that include nomenclatural acts results in losing the communication media (the names) with which we exchange and share the information on organisms, including information in GenBank. **J. Kojima**

Article 8.1.3.2 refers to having a fixed format and gives a few examples. I guess you are right to avoid mandating specific formats here – they will change over time in any case – but then this could lead to debates over whether a particular format is ‘widely accessible’. At some point someone could choose to publish a paper in a minority format which might pose issues for current and future accessibility – who is to say whether this is valid or not? To avoid such disputes you might choose to add a recommendation as to which formats would be preferred/acceptable at the date of writing, and in this instance you should probably use an external list e.g. from a suitable archive or standards body rather than dream up one of your own.

M. Jackson

Format issues and retrievability in the future – Text content and print

Technical progress is irresistible, and the practice of biological nomenclature is not an exception. The authors of the proposed amendment well appreciate this, as well as the connected problems. These problems include the threat, first, of complete loss of irreplaceable primary documents in view of the evidently insufficient reliability of the current internet facilities, and, second, of tampering with these documents. No doubt, these threats are real and very serious, and the contradiction between them and the urgent necessity to legalise online nomenclatural activity is acute, but brooks no delay in its resolution. Is there any exit? I believe yes. We should only address our own history, I mean the history of our discipline, biological nomenclature. Long before the first Codes were established, scientists kept seeking for means to make the nomenclature of living beings stable and reliable in spite of the abundance of taxonomists, with all the diversity of their views, habits and idiosyncrasies. And the necessary device was eventually discovered, the type principle.

I don't think it is reasonable to scrutinise here the functions of the type principle. I dare to hope that the vast majority of the people interested in the present issue do understand, or at least feel subconsciously the profound importance of, the implementation of the type principle. That is why I simply propose to seek for an equivalent of the type specimen as applicable to online publications containing nomenclatural acts.

The first to come to mind is a hard (paper) copy of an online publication, kept in a scientific library or the like, which must be specified as permanently connected with the applicable name (i) in the primary publication and (ii) in at least one online depository of scientific names (Zoological Record or the like). Certainly it is preferable if several to many identical paper copies of the primary document were stored in relevant depositories. It is a subject for further discussion if these hard copies are to be considered analogues of syntypes (of equal legal virtue) or of a holotype and paratypes, and if there should be several obligatory depositories where these copies are to be kept.

Fortunately we now have widespread and quick communication tools, which make efficient discussion possible, with clear and speedy results. We have at least three years before the XXI Zoological Congress (2012). Given 2009 for discussing the basic principles and seeking for the best form of the type equivalent, 2010 for elaborating the actual Code amendments based on the consensus about the basic principles and instruments, and 2011 for discussing these specific amendments, we have a chance to meet that Congress with a finalised proposal of the amended Code. In summary, I

feel it premature to discuss the present Proposal in detail before the basic principles are sufficiently discussed. **A.P. Rasnitsyn**

With an increasing number of taxonomic publications coming to BioMed Central's open-access online journals (including both *BMC Evolutionary Biology* and *Parasites & Vectors*), I would like to reaffirm how beneficial it would be for the ICZN to update the Code as soon as possible to reflect the fact that the primary mode of scientific communication is now online, rather than print. Not only are new scientific journals (such as those from BioMed Central and PLoS) commonly online only, but I am hearing on an increasingly regular basis about traditional journals announcing plans to drop print. Without question, a serious commitment to preservation of the scientific record is vital – not just for taxonomy, but for all research. Online publishers such as BioMed Central have shown that commitment, and as long as publishers are meeting best practices for digital preservation, it seems clearly desirable that these publications should 'count' as publications from the perspective of zoological nomenclature. Note that e-publication does not, by any means, rule out the use of paper as one form of preservation for 'born-digital' content, and it is a form of preservation that libraries archiving digital content are free to use. Digital publications, incorporating the right to reuse and create derivative works, offer many benefits for taxonomy and taxonomic databases, so it would be very unfortunate for the Code to continue to act as an obstacle for taxonomists keen to work with open-access journals. **M. Cockerill**

Format issues and retrievability in the future – Images

An earlier comment said '*As long as we have binary computers, we will also have at least ASCII, and very likely also UTF-8 and XML*'. This issue is not whether it is binary or not, but how much (or how complex) technology is required to get it from the stored form to the human form, which in this case is the alphanumericpunctobet. This is something we can probably keep in our sights with textual information, but when it comes to digital images and other multimedia there is no equivalent of ASCII and we are already in *deep* trouble. **J. Croft**

Archiving issues

As far as lightning strikes, etc., destroying electronic-only publications, this is easily dealt with. If even just a few dozen major libraries, herbaria, or museums established servers to mirror each other's contents, we could rest assured that our electronic documents are at least as secure as our paper libraries. I've recently found a paper, published in 1989, that does not exist in any of my local libraries, or even in the personal library of the author! Yet I can still find usenet postings from that era, redundantly archived in a number of locations. I think this will inevitably be a big win for libraries. The cost of running a server is a fraction of the subscription fee for a major journal. And if something should happen that causes the simultaneous loss of all the servers at libraries around the world, chances are good that nomenclatural priority will not be among our most pressing concerns. That's my little rant on why open access, electronic publishing is a good and inevitable thing. **T.W. Smith**

The large (physical) libraries are handicapped by the need for ever more (expensive) physical storage space, on the one hand, and by being few and far between (and thus

relatively inaccessible), on the other hand. More and more of the existing literature will migrate to the www, with a varying degree of accessibility. The large physical library is a threatened species. Still, the issue for the moment is how to go about creating an electronic format for this 'unalterable document published at a single point in time', something like a notarised PDF with an unassailable voucher of authenticity ('unaltered since . . .'), preferably readable by a ubiquitous (and simple and un-updatable) piece of software. Note that the issue is not only to establish priority, but that a publication also establishes other details, some of which will perhaps become important only in a future Code (the Codes being retroactive).

P. van Rijckevorsel

It would be very helpful in future if some authority is made responsible for keeping hard copies of the electronic publications. May it be the already existing Open Archival Information System (OAIS) or something similar. **S. Singh**

Once an electronic manuscript is published in our journal it is guaranteed permanency by being listed in PubMed Central and also by being deposited in digital archives in the University of Potsdam in Germany, at INIST in France and in e-Depot, the National Library of the Netherlands' digital archive of all electronic publications. Our journal also participates in the British Library's e-journals pilot project, and plans to deposit copies of all articles with the British Library. **C. Arme**

I think that allowing e-only publication is an extremely bad idea. There should be a stipulation that paper copies are deposited in five named public libraries as there is currently. Paper is proven to last hundreds or thousands of years, and electronic media are notoriously ephemeral. This is the view too of the British Library where I recently attended a meeting on archiving. It would be disastrous if this wasn't a requirement. People place too much trust in modern technology! **G. Beccaloni**

There isn't a clean dividing line between electronic and paper publication. If there is a requirement for a publication to be printed on acid free paper and lodged in, for example, ten libraries, it circumvents many of the issues with archival permanence. The problem with this may be in the inability of the libraries to cope with such an approach. Single papers are likely to be treated as manuscripts or reprints and with less care than 'real' journals. Furthermore, how would we know that the paper has been deposited correctly? From a digital perspective so long as the paper is open access anyone with an interest can keep a copy and print it if they like or archive it in their own databases. Like so many of these debates it tends to get polarised and distracted into talk of how long media types last etc., which is a red herring. Nobody has this debate about journals and the paper stock they use! I would suggest the solution is to allow electronic publication provided it is open access so that people can take copies and archive them. There are other requirements but these would be the key ones. Kill two birds with one stone. **R. Hyam**

Likelihood and economics of file migration

With all the talk of formats and migration of important documents, although we claim to be looking to the future, we are actually looking at the past. The volume of data is or will be so huge that it will be impossible to verify it all at each technology migration point, and glitches, once introduced, might not be detected and will be

propagated through future iterations. It was not perfect in the olden days and the equivalent were errors of translation and transcription – but the generation times were much much longer and there was always likely to be an ‘original’ somewhere to go back to. **J. Croft**

As I see it there are two aspects of this discussion: #1. Do we currently have the infrastructure for all-electronic taxonomic publications? #2. Will that infrastructure continue to exist in the centuries to come in a way that guarantees continual access to *all* such electronic publications? If #1 is not met, then #2 becomes redundant. However, I think that most will agree that the current infrastructure for electronic publications (taxonomic and otherwise) is at least as good as the one for paper publications, in which case, 1) can we assure that these publications will continue to exist in the future? and 2) will the publications still be accessible (i.e. will we have the hardware/software needed to read them)? [. . .] As I see it the solution to 1) is not one or two but many big electronic libraries where everything is stored. Memory (at least the electronic kind) is very cheap and there is no reason why there can't be one or more global electronic libraries in each country. Even if only 50% of the earth's countries had such libraries it would still leave us with 90 of them (how many copies of most early 19th century publications are still around?); and additional copies will still exist on personal computers, external hard drives etc. Lightning can strike, both literally and metaphorically, and it can hit ordinary libraries as well [. . .] so I think it is reasonable to say that electronic publications are as safe as paper ones. [. . .] As for 2), if there is a need and a demand for software that can read 50+ year old documents then that demand will be met, if not by the software companies, then by the scientific community itself. Much scientific software is continuously being developed by scientists rather than by Adobe, Microsoft and Apple and, though the future holds many uncertainties, I am convinced that trend will continue. **T. Simonsen**

What constitutes a publication? Who is a publisher?

This may be a silly question but ‘who is a publisher?’ I have two websites hosted by the American Museum for Natural History and the University of Nottingham respectively. The responsibility for content, however, lies with myself. Presently the sites contain material relating to what appear to be new species but I accept that under the existing rules none constitute formal publication. The new rules, however, seem to suggest that if I submitted the new names for registration they would become valid. The problem but also great merit of website writing is the capacity to move knowledge along at a rapid rate. I, for instance, have been overwhelmed with specimens from ecological collections and the very time-consuming process of preparing and submitting manuscripts for journals, such as *Zootaxa*, with differing styles and formats, is a real negative. **B. Taylor**

The ICZN has not promoted that some journals (*BMC Evolutionary Biology*, *BMC Microbiology*, *Parasites and Vectors*, *Frontiers in Zoology*, *PLoS ONE*) have already been publishing new names in an electronic-only form. Also we haven't promoted that most journals of professional publishers (many thousands, not only the several mentioned in this discussion) are published online many months in advance to the print version. It just happens. It happens because the scientific community seems to

prefer rapid communication. The Amendment tries to find a solution for this situation by giving guidelines for publication practices that are currently in place without guidelines. If we get a PDF of a publication, how do we know whether a paper copy exists? Do we ever check the original paper copy before citing the PDF for nomenclatural purposes? Most of us are far away from a comprehensive library. With increasing replacement of reprints by PDFs and increasing proportion of electronically delivered interlibrary loans, it is difficult to determine whether a paper copy exists. We just assume. **F. Krell**

It sounds as though anything that someone can get appropriately archived and accessible online would be nomenclaturally legal. This is already a problem with essentially self-published paper volumes; allowing it online would only make it easier for someone to cause lots of headaches and extra work for serious taxonomists by legalising a lot of poorly justified names. (Of course, not all self-published materials are of poor quality, but I suspect almost all of us can think of someone who (in our opinions) could advance the science of taxonomy by ceasing to publish taxonomic work). **D. Campbell**

The future of publishing small journals and survival of taxonomic societies

It seems self-evident that this system would be the death of entomological societies that publish journals heavily taxonomic in orientation. That affects all of us, well beyond those printing houses rolling in profits from taxonomy (yes, that was sarcasm), including students who might have become interested in the field. I also believe that the specific assertion that ‘most relevant is the fact that it would put taxonomy (again) in its central scientific position’ is completely incorrect. More relevant is the fact that taxonomy would become a sequestered science, irrelevant to and unread by anyone not doing taxonomy. Taxonomy would completely lose the readership of all the journals that currently publish taxonomic work. Out-of-sight, out-of-mind. Taxonomy would hardly be central to science in that context – rather, it would be its own private club. **S.D. Gaimari**

In response to discussion of a proposed open-wiki, single-outlet system for all taxonomy: I am very strongly in favour of this amendment. Contrary to some arguments, new taxon descriptions published electronically will be much more readily available to researchers around the world than are print versions. Passing of this amendment will also allow journals that are struggling financially because of the requirement for hard copies of descriptions to go entirely electronic, which may allow them to continue to exist rather than having to fold. **H.C. Proctor**

Case 3475***Myrmarachne* MacLeay, 1839 (Araneae, SALTICIDAE): proposed conservation of the generic name**

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Abstract. The purpose of this application, under Article 23.9.3 of the Code, is to conserve the generic name *Myrmarachne* MacLeay, 1839 for a well-known and widespread group of extant, ant-mimicking jumping spiders (Araneae, SALTICIDAE) by suppression of a little-used senior subjective synonym, *Entomocephalus* Holl, 1829, introduced for a fossil spider preserved in either Eocene Baltic amber or perhaps in Subrecent copal. Holl (1829) based this name on drawings in an earlier work by Schweigger (1819). This original description did not state the number of specimens, but only one was figured, which we presume to represent the holotype by monotypy. Its repository could not be traced and we are not aware of subsequent specimens which could provide a neotype. However, the illustration of an ant-like salticid by Schweigger is of sufficient quality that we consider it likely that *Entomocephalus* and *Myrmarachne* are synonyms.

Keywords. Nomenclature; taxonomy; Araneae; SALTICIDAE; *Myrmarachne*; *Entomocephalus*; *Myrmarachne melanocephala*; salticid spider; Baltic amber.

1. Schweigger (1819) wrote a treatise on the anatomy and physiology of corals, which included an appendix with observations on amber. The origin of this amber is not clearly stated, but since August Friederich Schweigger was professor in Königsberg (now Kaliningrad) it would implicitly be Baltic amber of Palaeogene (Eocene) age (but see below). Schweigger (1819) illustrated some insects, a scorpion and (pl. VIII, figs. 68, 68a) a spider, none of which were formally named. Schweigger's drawings are nevertheless quite good and the spider shown is evidently a member of the SALTICIDAE (jumping spiders), as shown by the large anterior median eyes and characteristic arrangement of the other eyes. Also notable in the drawing are the massive, forward-projecting chelicerae with a series of internal teeth (or spines) on the basal article and a long, slightly S-shaped, slender fang with a distally curved tip, as well as subdivision of the prosoma to give the body a tripartite, and distinctly ant-like, form.

2. In a subsequent early palaeontological textbook Friederich Holl introduced the name *Entomocephalus formicoides* Holl, 1829 (pp. 178–9) for this fossil, believing it to

be some sort of cross between an insect and a spider. Neither Schweigger nor Holl clearly stated the repository of the type material, although presumably this was either a university or museum collection in Königsberg. In the same textbook, Schweigger's amber scorpion was named *Scorpio schweiggeri* Holl, 1829. Its type specimen was explicitly reported as lost by Lourenço & Weitschat (1996) and we fear that this is the case for the type specimen of *E. formicoides* too. Other jumping spiders are known from Baltic amber (e.g. Koch & Berendt, 1854; Żabka, 1988; Wunderlich, 2004b), but none exhibit the distinctive morphology which could make them suitable as a neotype for this species.

3. The generic name *Myrmarachne* MacLeay, 1839 (p. 10) was introduced for a Recent, ant-mimicking, jumping spider from Bengal. The type species of *Myrmarachne* MacLeay, 1839 is *Myrmarachne melanocephala* MacLeay, 1839 (p. 11), by monotypy. MacLeay's original diagnosis of the genus refers to its long, projecting 'antennae' [= chelicerae] with a series of spines on the basal article and a long 'sinuous' fang. MacLeay's diagnosis thus matches Schweigger's amber fossil almost perfectly. The only minor difference is the number of cheliceral teeth: six in the original diagnosis compared to seven according to Schweigger's illustration. Despite the absence of fossil type material, we see little reason to doubt that *Entomocephalus* is a synonym of *Myrmarachne*. The modern genus has a cosmopolitan distribution with over 200 Recent species, the majority of which are found in the tropics of Africa, the Austro-Pacific region and Asia. No fossils have been assigned to this genus.

4. Since its original description, *Entomocephalus* has only been mentioned on six further occasions that we are aware of, but occasionally after 1900, which precludes the automatic application of Article 23.9.1.1. Geinitz (1846, p. 192) noted it in an early textbook on palaeontology and Scudder (1891, p. 261) included it in his compilation of fossil terrestrial arthropods. Petrunkevitch (1955, p. 152) listed the name, without comment, as incertae sedis in the *Treatise on Invertebrate Paleontology*. Subsequently, he listed it (Petrunkevitch, 1958, p. 372) as a questionable member of another spider family, ARCHAEIDAE Koch & Berendt, 1854. Penney (2003, p. 126) recognised that *E. formicoides* is an ant-mimicking jumping spider, explicitly suggesting that it '...is almost certainly a salticid, probably belonging to the genus *Myrmarachne*...' and further noted that *Entomocephalus* would, under these circumstances, be the older name. Finally, Wunderlich (2004a, p. 34) mentioned *E. formicoides* as a '...striking old fake...', raising the possibility that the fossil does not derive from Baltic amber, but from Subrecent Madagascan copal. In this context Wunderlich's use of 'fake' implies a genuine subfossil specimen in copal being passed off as considerably older amber (J. Wunderlich, pers. comm. 2008). Wunderlich agreed that this specimen is probably a *Myrmarachne* and that there was precedent for the occurrence of this genus in copal (cf. Goeppert & Berendt, 1845), albeit under an older preoccupied name *Pyrophorus* Koch, 1837 – incorrectly spelled '*Poryphorus*' in Wunderlich's paper.

5. The name *Myrmarachne* is common and widespread both in the scientific (cf. Platnick, 2008 and citations therein) and popular literature on spiders and thus satisfies the conditions of Article 23.9.1.2. It is also in use in standard online spider databases (Prószyński 2007; Shorthouse 2008). Recent usages include descriptions of new taxa and/or revisions by Wanless (1978), Berry et al. (1996) and Wesłowska & Salm (2002), in faunistics (Bradley et al., 2006) and in comprehensive studies of

jumping spider phylogeny (e.g. Maddison & Hedin, 2003; Maddison et al., 2008). *Myrmarachne* has been widely used in reviews of ant-mimicry by spiders (Cushing 1997; and references therein) and the genus is regarded as a model for Batesian mimicry (Nelson et al., 2006; Ceccarelli & Crozier, 2007). The name has also been used in further studies of ant-associations and the general natural history of these remarkable-looking spiders (Edmunds, 2006; Jackson et al., 2008). A further list of citations of the name *Myrmarachne* in various biological fields (taxonomy, phylogeny, behaviour) is available at <http://research.amnh.org/entomology/spiders/catalog/INTRO2.html> (Platnick's Catalogue). Replacing the name *Myrmarachne* with the senior, but little-known fossil name *Entomocephalus* would cause considerable confusion and instability. It is therefore proposed that the name *Entomocephalus* Holl, 1829 be suppressed.

6. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to suppress the generic name *Entomocephalus* Holl, 1829 for the purposes of the Principle of Priority but not for those of the Principle of Homonymy;
- (2) to place on the Official List of Generic Names in Zoology the name *Myrmarachne* MacLeay, 1839, type species by monotypy *Myrmarachne melanocephala* MacLeay, 1839;
- (3) to place on the Official List of Specific Names in Zoology the name *melanocephala* MacLeay, 1839, as published in the binomen *Myrmarachne melanocephala*, the specific name of the type species of *Myrmarachne* MacLeay, 1839;
- (4) to place on the Official Index of Rejected and Invalid Generic Names in Zoology the name *Entomocephalus* Holl, 1829, as suppressed in (1) above.

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Comments on this case are invited for publication (subject to editing) in the *Bulletin*; they should be sent to the Executive Secretary, I.C.Z.N., c/o Natural History Museum, Cromwell Road, London SW7 5BD, U.K. (e-mail: iczn@nhm.ac.uk).

Case 3467

ZOSIMIDAE Seifried, 2003 (Crustacea, Copepoda, Harpacticoida): proposed emendation of spelling to ZOSIMEIDAE to remove homonymy with ZOSIMINAE Alcock, 1898 (Crustacea, Decapoda, XANTHIDAE)

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Abstract. The purpose of this application, under Articles 29 and 55.3.1 of the Code, is to remove homonymy between the family-group names ZOSIMINAE Alcock, 1898 (Crustacea, Decapoda) and ZOSIMIDAE Seifried, 2003 (Crustacea, Copepoda) by changing the spelling of the junior homonym. It is proposed that the entire name *Zosime* Boeck, 1873 (Copepoda) be used to form ZOSIMEIDAE, leaving the stem of the senior homonym (based on the name *Zosimus* A.-G. Desmarest, 1823; Decapoda) unchanged. *Zosimus* A.-G. Desmarest, 1823 and *Zosime* Boeck, 1873 are respectively the type genera of ZOSIMINAE Alcock, 1898 (Decapoda) and ZOSIMIDAE Seifried, 2003 (Copepoda).

Keywords. Nomenclature; taxonomy; Crustacea; Decapoda; Copepoda; Harpacticoida; XANTHIDAE; ZOSIMEIDAE; ZOSIMIDAE; ZOSIMINAE; *Zosime*; *Zosimus*; *Zosime typica*; cosmopolitan.

1. Leach (1818) introduced the French vernacular names ‘Carpile’, ‘Clodorée’ (sic) and ‘Zosime’ for three genera of decapod crustaceans but did not include a description, definition or indication of the taxa they denoted (Leach, 1818, pp. 74–75). Under Article 12 Leach’s names are nomina nuda and must be considered unavailable.

2. A.-G. Desmarest (1823, p. 228) latinised Leach’s (1818) vernacular names in a footnote to his text dealing with the genus *Cancer*, naming them *Carpilius*, *Clorodius* and *Zosimus*, respectively. This footnote was essentially repeated verbatim in A.-G. Desmarest (1825, pp. 104–105). Through correspondence with Pierre André Latreille (his colleague at the veterinary school at Alfort near Paris) A.-G. Desmarest was aware of an unpublished manuscript (‘Dans un travail qui n’a pas été publié, M. Leach a formé ...’) in which Leach introduced the three latinised genus-group names. Leach resigned from the British Museum on 9 March 1822 and never published the manuscript (Harrison & Smith, 2008). Under Article 50, authorship and date of the generic names *Carpilius*, *Clorodius* and *Zosimus* must be attributed to A.-G. Desmarest (1823) since he was the person who first published them in a way that satisfied the criteria of availability. Subsequent treatment of *Carpilius* by various authors and the Commission poses problems, the resolution of which will be proposed separately.

3. As for *Zosimus*, the original description by A.-G. Desmarest (1823, p. 228) is concise (‘... *Zosimus* ... est voisin des xanthes, et ... comprend le *Cancer æneus* et quelques autres espèces dont les pieds sont un peu aplatis’) but effectively designates

Cancer aeneus Linnaeus, 1758 as the type species by monotypy. E. Desmarest's (1858, p. 17) subsequent designation of '*Cancer rugosus* Fabr.' as type species of '*Zozymus*' is not only invalid but also refers to an incorrect binominal name and authorship. The French vernacular name (Zozyme ridé) cited by E. Desmarest (1858) suggests that he meant the species described as *Zozymus rugatus* H. Milne Edwards, 1834 (p. 385) (and denoted by the same vernacular name) now placed in the genus *Liomera* Dana, 1851.

4. Despite the history recounted above, the generic name *Zosimus* was placed on the Official List of Generic Names in Zoology in Opinion 85 (16 December 1925) and attributed to 'Leach in Desmarest, 1823' (*Smithsonian Miscellaneous Collections*, 73(3): 13–18). Later, in Direction 37 (Completion of the entries relating to names of certain genera of the Order Decapoda (Class Crustacea) placed on the *Official List of Generic Names in Zoology* in the period up to the end of 1936; 1 September 1956), the name *Zosimus* was attributed to either 'Leach, 1823' (pp. 53, 69) or 'Leach, 1825' (pp. 61, 77) without reference to A.-G. Desmarest at all (*Opinions and Declarations* 1(Section D, Part D.2): 47–82).

5. The nomenclatural history of the genus *Zosimus* is riddled with subsequent incorrect spellings, which in many cases led to the erroneous citation of family-group names based on them. As was noted above, Henri Milne Edwards (1834, p. 383) used the spelling *Zozymus* for *Zosimus*, referring to it in a footnote as '*Zozymus* Leach'. This subsequent spelling was adopted by many authors (e.g. Miers, 1886, p. 134; Henderson, 1893, p. 359; Alcock, 1898, p. 103; Balss, 1922, p. 124; Sakai, 1939, p. 450; Ward, 1942, p. 85; Tweedie, 1950, p. 115). The Commission (Direction 37, 1 September 1956) ruled that *Zozymus* Milne Edwards (H.) was an incorrect subsequent spelling for *Zosimus* Leach, 1825 (*Opinions and Declarations* 1(Section D, Part D.2): 47–82), but the name nevertheless continued to be in wide usage (e.g. Sankarankutty, 1961, p. 128; Edmondson, 1962, p. 225; Chang, 1963, p. 98; Michel, 1964, p. 32; Kensley, 1981, p. 46). Other incorrect subsequent spellings of *Zosimus* include *Zozimus* by Boone (1934, p. 99), *Zoozymus* by Balss (1938, p. 38) and *Zosymus* by Serène (1968, p. 73), which was subsequently adopted by Sakai (1976, pp. 402–403).

6. Alcock (1898, p. 77) subdivided the subfamily XANTHINAE into six 'Alliances', which would nowadays be attributed infrasubfamilial rank similar to that of tribe. In the ZOZYMOIDA (based on the incorrect subsequent spelling *Zozymus*) he included the genera *Atergatis* de Haan, 1833, *Lophactaea* A. Milne Edwards, 1862, *Zozymus* Leach in Desmarest, 1823 and *Lophozozymus* A. Milne Edwards, 1863 (Alcock, 1898, p. 94). The next reference to this family-group name is Stimpson's (1907, p. 57) posthumous publication (prepared in 1858), which makes mention of a taxon ZOZYMINAE. Both Serène (1968, p. 73) and Sakai (1976, pp. 385, 401) continued to use ZOZYMOIDA while Takeda (1976, p. 70) followed Stimpson (1907) in attributing subfamilial rank to the taxon (as ZOZYMINAE). Family-group names based on incorrect subsequent spellings are to be corrected (Article 32.5.3.3 of the Code). Serène (1984, p. 137) corrected the spelling to ZOSIMINAE in accordance with the original spelling of the type genus and placed the subfamily in the XANTHIDAE, but erroneously attributed authorship to 'Stimpson, 1907'. Števíć (2005, p. 44) effectively cited ZOSIMINI Alcock, 1898 as one of nine tribes recognised within the subfamily XANTHINAE and listed *Zosimus* (attributed to Leach, 1818) as the type genus. In recent years, authors (e.g. Clark & Ng, 1998; Clark et al., 2004; Poore,

2004) have treated the taxon as a subfamily ZOSIMINAE in the XANTHIDAE and this rank is also used in Ng et al. (2008, pp. 30, 205).

7. Boeck (1873, p. 45) introduced the generic name *Zosime* for a new genus of harpacticoid copepods in a publication completely lacking in illustrations. The only originally included species was *Zosime typica* Boeck, 1873, which is the type species by monotypy. A detailed re-description was provided by Sars (1903, pp. 27–28, Plate XV) based on material from the type locality (Oslofjord). Members of *Zosime* are common representatives of the harpacticoid communities in deepwater benthic habitats (e.g. Fiers, 1991; Huys et al., 1992; Ahnert & Schriever, 2001; Baguley et al., 2006) and several authorities have published species identification keys for the genus (Bodin, 1968; Coull, 1973; Dinert, 1974). According to Wells (2007, p. 73) *Zosime* currently accommodates 14 valid species and two subspecies, but it is known that several new species await description (e.g. Carey & Montagna, 1982; Kotwicki, 2002; Mu et al., 2002; Seifried, 2003).

8. Sars (1903, p. 26) placed the genus *Zosime* Boeck, 1873 in the family CERVINIIDAE Sars, 1903 (a junior synonym of AEGISTHIDAE Giesbrecht, 1893) but this assignment was disputed by Lang (1944, p. 10; 1948, p. 361), who transferred the genus to a new subfamily IDYANTHINAE Lang, 1944 in the family TISBIDAE Stebbing, 1910. Por (1967, p. 123) attributed *Zosime* Boeck, 1873 to the family TACHIDIIDAE Boeck, 1865 but this placement is almost certainly a lapsus calami. The family-group name ZOSIMIDAE was first introduced as an nomen nudum by Huys et al. (1992, Table 2 & p. 37) in an ecological paper. Although the taxa ‘ZOSIMIDAE (*Zosime*, *Tachidiella*)’ and ‘IDYANTHIDAE (*Idyella*, *Idyanthe*, *Tachidiopsis*)’ were treated as separate entities in Huys et al.’s (1992) analysis, under Article 13.1 of the Code, the nomenclatural impact of this act is nil. Seifried (2003, p. 100) established a new family ZOSIMIDAE for the genera *Zosime* Boeck, 1873, *Peresime* Dinert, 1974 and *Pseudozosime* T. Scott, 1912, which she had removed from the former tisbid subfamily IDYANTHINAE.

9. ZOSIMIDAE Seifried, 2003 (type genus *Zosime* Boeck, 1873) (Copepoda) is a junior homonym of ZOSIMINAE Alcock, 1898 (type genus *Zosimus* A.-G. Desmarest, 1823 (Decapoda)). Under Article 55.3.1 the homonymy between ZOSIMINAE Alcock, 1898 and ZOSIMIDAE Seifried, 2003 must be referred to the Commission. We propose that the entire generic name *Zosime* Boeck, 1873 be adopted as the grammatical stem, so that the copepod family name will become ZOSIMEIDAE and the homonymy will be removed.

10. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to rule that for the purposes of Article 29 of the Code the stem of the generic name *Zosime* Boeck, 1873 is *Zosime*-;
- (2) to place on the Official List of Generic Names in Zoology the name *Zosime* Boeck, 1873 (gender: feminine), type species by monotypy *Zosime typica* Boeck, 1873 (Crustacea, Copepoda);
- (3) to place on the Official List of Specific Names in Zoology the name *typica* Boeck, 1873 as published in the binomen *Zosime typica* (specific name of the type species of *Zosime* Boeck, 1873) (Crustacea, Copepoda);
- (4) to place on the Official List of Family-Group Names in Zoology the following names:
 - (a) ZOSIMINAE Alcock, 1898, type genus *Zosimus* A.-G. Desmarest, 1823 (Crustacea, Decapoda);

- (b) ZOSIMEIDAE Seifried, 2003, type genus *Zosime* Boeck, 1873 (spelling emended by the ruling in (1) above) (Crustacea, Copepoda);
- (5) to place on the Official Index of Rejected and Invalid Family-Group Names in Zoology the name ZOSIMIDAE Seifried, 2003 (an incorrect original spelling of ZOSIMEIDAE, as ruled in (1) above) (Crustacea, Copepoda).

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Case 3484

NOMIIDAE Gozis, 1875 (Insecta, Coleoptera): proposed emendation of spelling to NOMIUSIDAE to remove homonymy with NOMIINAE Robertson, 1904 (Insecta, Hymenoptera)

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Abstract. The purpose of this application, under Articles 29 and 55.3, is to emend the family-group name NOMIIDAE Gozis, 1875 (Insecta, Coleoptera), a senior homonym of NOMIINAE Robertson, 1904 (Insecta, Hymenoptera), long-considered a synonym of PSYDRINA LeConte, 1853, thereby removing the homonymy between the two names. It is proposed that the stem of the generic name *Nomius* Laporte, 1835, on which the beetle family-group name is based, be emended to give NOMIUSIDAE, while leaving the bee family-group name (based on *Nomia* Latreille, 1804) unaltered.

Keywords. Nomenclature; taxonomy; Hymenoptera; Coleoptera; CARABIDAE; NOMIINAE; NOMIUSIDAE; *Nomia*; *Nomius*; *Nomia curvipes*; *Nomius graecus*; *Morio pygmaeus*; ground beetles; sweat bee; Nearctic; Palearctic; Afrotropical; Regions; Asia; Australia.

1. Latreille (1804, p. 182) established the genus *Nomia* for a group of bees. The type species is *Andrena curvipes* Fabricius, 1781 by monotypy.

2. Laporte (1835, p. 144) established the genus *Nomius* for a group of ground beetles. The type species is *Nomius graecus* Laporte, 1835 (p. 145) (= *Morio pygmaeus* Dejean, 1831) by monotypy. The only species originally included in the genus *Nomius* by Laporte, 1835 was *Nomius graecus*; thus it is the valid type species of this genus. *Nomius graecus* Laporte, 1835 is a junior synonym of *Morio pygmaeus* Dejean, 1831 (p. 512), which is the valid name for the type species of *Nomius*.

3. Gozis (1875, p. 3) established the family NOMIIDAE for beetles of the genus *Nomius* Laporte, 1835. Although not presently in use the name is available and was used as valid in the classification of CARABIDAE Latreille, 1802 by some authors (e.g. Lindroth, 1961 as NOMIINI). The older name PSYDRINA LeConte, 1853 is the presently valid name for this taxon (Madge, 1989; Baehr, 1998; Hürka, 2003; Lorenz, 2005).

4. Robertson (1904, p. 42) established the family NOMIIDAE for bees of the genus *Nomia* Latreille, 1804 and related genera. In subsequent classifications the group has been considered a subfamily of the HALICTIDAE Thomson, 1869 (e.g. Michener, 1944; Engel, 2005).

5. The name NOMIIDAE Gozis, 1875 pre-dates the junior homonym NOMIINAE Robertson, 1904 by more than a quarter century. Today the bee family-group name is widely used in the literature for a group of 532 living species (e.g. Michener, 1944, 2007; Rozen, 1986; Moure & Hurd, 1987; Pauly, 1990; Wcislo, 1993; Radchenko & Pesenko, 1994; Wcislo & Engel, 1997; Engel, 2001, 2005; Baker, 2002; Melo & Gonçalves, 2005; Astafurova, 2008), while the beetle family-group name is considered a junior synonym. No existing synonym is available to replace the bee name and it would therefore require the establishment of a new family-group name. Such a change would disrupt the extensive literature on the systematics and biology of bees and would be contrary to overall nomenclatural stability. We therefore propose to remove this homonymy by using the entire generic name *Nomius* as the stem for the formation of the unused beetle family name, leaving the bee name unaltered. Such an emendation would not affect the availability of the beetle family-group name should it someday be recognised as a valid taxonomic group.

6. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary powers to rule that for the purposes of Article 29 of the Code the stem of the generic name *Nomius* Laporte, 1835 is *Nomius*–;
- (2) to place on the Official List of Generic Names in Zoology the following names:
 - (a) *Nomius* Laporte, 1835 (gender: masculine), type species by monotypy *Nomius graecus* Laporte, 1835;
 - (b) *Nomia* Latreille, 1804 (gender: feminine), type species by monotypy *Andrena curvipes* Fabricius, 1781;
- (3) to place on the Official List of Specific Names in Zoology the following names:
 - (a) *pygmaeus* Dejean, 1831, as published in the binomen *Morio pygmaeus* (valid name of the type species of *Nomius* Laporte, 1835);
 - (b) *curvipes* Fabricius, 1781, as published in the binomen *Andrena curvipes* (specific name of the type species of *Nomia* Latreille, 1804);
- (4) to place on the Official List of Family-Group Names in Zoology the following names:
 - (a) NOMIUSIDAE Gozis, 1875, type genus *Nomius* Laporte, 1835 (spelling emended by the ruling in (1) above);
 - (b) NOMIINAE Robertson, 1904, type genus *Nomia* Latreille, 1804 (Insecta, Hymenoptera);
- (5) to place on the Official Index of Rejected and Invalid Family-Group Names in Zoology the name NOMIIDAE Gozis, 1875 (spelling emended to NOMIUSIDAE, as ruled in (1) above) (Insecta, Coleoptera).

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Case 3463

***Testudo gigantea* Schweigger, 1812 (currently *Geochelone* (*Aldabrachelys*) *gigantea*; Reptilia, Testudines): proposed conservation of usage of the specific name by maintenance of a designated neotype, and suppression of *Testudo dussumieri* Gray, 1831 (currently *Dipsochelys dussumieri*)**

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Abstract. The purpose of this application, under Article 75.8 of the Code, is to conserve the specific name *Testudo gigantea* Schweigger, 1812 in its customary usage for the giant land tortoise (family TESTUDINIDAE) found on Aldabra Atoll in the western Indian Ocean. Taxonomic and nomenclatural confusion in the 19th and early 20th centuries led to competing and contradictory proposals between 1982 and 2006 to supplant *T. gigantea* Schweigger, 1812, first with *T. elephantina* Duméril & Bibron, 1835 and then with *T. dussumieri* Gray, 1831. As a corollary, *Aldabrachelys* Loveridge & Williams, 1957, the genus-group name erected for the Aldabra tortoise, was supposedly reduced to a junior synonym of either the extinct Mascarene genus/subgenus *Cylindraspis* Fitzinger, 1835, or the South American genus/subgenus *Chelonoidis* Fitzinger, 1835, and replaced as the name of the Aldabra tortoise by the generic name *Dipsochelys* Bour, 1982. To stabilise the accustomed name of this species as *Geochelone* (*Aldabrachelys*) *gigantea*, a neotype of *T. gigantea* Schweigger, 1812 was designated in 2006. A subsequent claim of the rediscovery of the long-lost holotype now again threatens this usage and stable nomenclature by (1) rendering *T. gigantea* a junior subjective synonym of *T. denticulata* Linnaeus, 1766, (2) resurrecting the former nomen oblitum *Testudo dussumieri* Gray, 1831 for the Aldabra tortoise, and (3) setting aside the neotype of *T. gigantea*; this action also again threatens the established use of *Aldabrachelys*. The supposed holotype rediscovery is not unequivocally proven, and for the sake of nomenclatural universality and stability, it is proposed that the neotype designation of 2006 be affirmed and that *T. dussumieri* Gray, 1831 be suppressed.

Keywords. Nomenclature; taxonomy; Testudines; TESTUDINIDAE; *Aldabrachelys*; *Chelonoidis*; *Cylindraspis*; *Dipsochelys*; *Geochelone*; *Testudo*; *Testudo carbonaria*; *Testudo denticulata*; *Testudo dussumieri*; *Testudo elephantina*; *Testudo gigantea*; land tortoises; Aldabra Atoll.

1. Schweigger's (1812) much cited and debated publication included descriptions of seventeen species of land tortoises (TESTUDINIDAE), all considered at the time as being members of the genus *Testudo* Linnaeus, 1758 (Bour, 2008, p. 14). Among them, three new species of *Testudo* were described; of these, *Testudo gigantea* Schweigger,

1812, pp. 327, 362 (description republished in part, Schweigger, 1814, p. 58; see Bour, 2008, p. 16) has resulted in a great deal of nomenclatural and taxonomic debate, notably since 1982 when the established nomenclature was challenged by Bour (1982).

2. The original description of *Testudo gigantea* was based on a holotype (by monotypy) reported by Schweigger to be 'in the Paris Museum' (evidently the Muséum National d'Histoire Naturelle, Paris, 'MNHN') and said to have come from the royal collection of Lisbon; the locality was listed as 'Brasilia' (see also Bour, 1982, p. 117; 1984a, p. 163; 2006, p. 13; Crumly, 1986, p. 238; Pritchard, 1986, p. 522). Fourteen measurements of the specimen were provided; 'Longitudo testae' (shell length) was reported as '2 ped. 4 poll.' (Schweigger, 1812, p. 363), which may refer to the length of the carapace over the curve and is reckoned to have been equivalent to 756 mm (Bour, 1984a, pp. 165, 166; 2006, p. 18). Schweigger's (1812, p. 362) original description in 19th century Latin also included details about the shape of various body parts and the sculation. Interpretations of Schweigger's (1812) description of *Testudo gigantea* vary among modern authors (cf. Bour, 1982, p. 117; 1984a, pp. 163, 165, 169; 2006, pp. 12–13, 16; Crumly, 1986, p. 238; Pritchard, 1986, pp. 522, 524, 526). Schweigger's original description, while detailed for its time, does not allow unequivocal interpretation (Crumly, 1986, pp. 237–238; Pritchard, 1986, pp. 523–528); it does not provide diagnostic features for distinguishing his specimen of *T. gigantea* from other modern species of large, or giant, tortoises, several of which are known from various localities on islands in the western Indian Ocean and Galapagos Archipelago, as well as from continental Africa and South America.

3. Other species of land tortoise discussed and recognised by Schweigger were '*Testudo denticulata* Linnaeus,' and '*Testudo tabulata* Wallbaum'[sic] (Schweigger, 1812, pp. 322, 324, 444–445, 452–453), with specimens of both species reported from the Paris Museum. Because Walbaum (1782, p. 122) used the non-binominal combination '*Lorica testudinis tabulatae*,' his name is unavailable, and the name *Testudo tabulata* only became available after it was published by Schoepff (1792 pp. 56–63). It is regarded as a junior synonym of *T. denticulata* Linnaeus, 1766 (Wermuth & Mertens, 1961, p. 189; Fritz & Havaš, 2007, p. 270), now classified as *Geochelone* (*Chelonoidis*) *denticulata* (Linnaeus, 1766, p. 352) and known from eastern Brazil, elsewhere in South America, and Trinidad Island. Schweigger's (1812, p. 322) report of a single shell of '*Testudo tabulata africana*' in the Paris Museum was presumably based on Schoepff (1792, p. 59; also published as Schöpfung) who had given the provenance of *T. tabulata* as southern Africa in error (Fritz & Havaš, 2007, p. 270).

4. J.E. Gray (1831a, p. 3; 1831b, p. 9) placed *Testudo gigantea* Schweigger, 1812 within 'Var. γ ' which was one of three 'varieties' that he listed within the synonymy of '*Testudo indica* Gmel. Fide Perrault.' Also included within Gray's (1831a, p. 3) synonymy of *T. indica* was '*Test. Dussumieri*, Schegel'[sic]. and '*Test. Dussumieri*, Schlegel MSS' (1831b, p. 9). Under Article 50.7 of the Code, Gray (1831b) is deemed to be the author of this name (which he used as a synonym), even though another originator (Schlegel) was cited (see also Fritz & Havaš, 2007, p. 265).

5. Duméril & Bibron (1835) recognised and discussed 22 species in the genus *Testudo*, including *Testudo gigantea* Schweigger, 1812, based on a specimen significantly larger than Schweigger's type (Duméril & Bibron, 1835, pp. 120–123). This

was the only specimen of this species reported from the Paris Museum, both in 1835 and nearly 20 years later (Duméril & Duméril, 1851, p. 5): it was from an unknown locality. Bour (1984a, p. 169; 2006, p. 13) concluded that specimen MNHN 9566, recently catalogued in the Muséum National d'Histoire Naturelle, Paris, was the specimen that Duméril & Bibron (1835) treated as *T. gigantea*. Bour (1982, p. 117; 1984a, pp. 168–169; 2006, p. 13) claimed that although Duméril & Bibron (1835, pp. 120–123) used Schweigger's name, *T. gigantea*, they actually had a new and different species: the Aldabra tortoise.

6. Duméril & Bibron (1835, pp. 89–98) also recognised and described '*T. tabulata* Walbaum, 1782', including *Testudo denticulata* Linnaeus, 1766 within the synonymy. In a discussion of confusions among tortoise species by previous authors, Duméril & Bibron (1835, p. 98), who instructed Schweigger during his visit to Paris (Duméril & Bibron, 1834, p. 416; Bour 1984a, p. 162 footnote 2; 2006, p. 19; 2008, pp. 7, 11; Adler, 2007, p. 140), stated unequivocally that '... *T. gigantea* of Schweigger does not bear the slightest resemblance to either *T. hercules* Spix or Tortue Marqueté' (their common name for *T. tabulata*); both of these last two binomens are today recognised as junior synonyms of *Geochelone* (*Chelonoidis*) *denticulata* (Linnaeus, 1766) (Fritz & Havaš, 2007, p. 270). Bour (2006, p. 19) asserted that MNHN 9554, reported to have a 19th century identification of '*T. carbonaria* Dum. Bib' and recently catalogued in the Muséum National d'Histoire Naturelle, Paris, as '*Chelonoidis denticulata* (Linnaeus, 1766)', is the same specimen that Duméril & Bibron (1835) reported on as *Testudo tabulata*. Bour (2006) further asserted that MNHN 9554 is the holotype of *T. gigantea* Schweigger, 1812.

7. Duméril & Bibron (1835, pp. 99–102) distinguished '*Testudo carbonaria* Spix' from '*Testudo tabulata* Walbaum'. This is notable because they reported that the two nominal species had the same geographic range, and for over a century *T. carbonaria* Spix, 1824 was commonly confused with *T. denticulata* Linnaeus, 1766, species that today are regarded as distinct (Williams, 1960; Fritz & Havaš, 2007, p. 268–269).

8. Duméril & Bibron (1835, p. 110 and following pages) also described a new species, *T. elephantina*; in doing so, they specifically distinguished it from Gray's (1831b, p. 9) *T. indica* 'Var. γ ,' namely *T. gigantea* Schweigger, 1812. The locality of the species was reported to be islands in the 'Mozambique Channel,' particularly Anjouan, 'Aldebra'[sic], and the Comoros, but it was recognised by Duméril & Bibron that specimens of this species were commonly transhipped through Mauritius and Reunion Islands in the Mascarene Archipelago, which often caused misunderstandings about original distributions. Hence, the type series of *T. elephantina* in the Paris Museum was composed of specimens from the Mascarenes (Duméril & Bibron, 1835, p. 114; Rothschild, 1915, pp. 425–426; Bour, 1984a, p. 171). Rothschild (1915, pp. 425, 432) designated the largest specimen, measured by Duméril & Bibron (1835), as the 'type' (Article 74.5 of the Code); and Bour (1984a, p. 171; 1984b, p. 291) affirmed that the lectotype from the type series of *T. elephantina* was MNHN 7874.

9. During the 19th and early part of the 20th century, there was considerable confusion about how many species of tortoise occurred in the Seychelles, particularly on Aldabra Atoll, and which species name were correct (Bour, 1984a; Frazier, 2006a). Duméril & Bibron (1835, p. 120), Günther (1877, p. 22, footnote), Hubrecht (1881, p. 43) and Boulenger (1889, p. 168) regarded *Testudo gigantea* Schweigger,

1812 as similar to – and possibly conspecific with – *Testudo elephantina* Duméril & Bibron, 1835 (see also Bour, 1984a, p. 169; 2006, p. 15). Boulenger (1894, p. 305) referred to ‘the true *Testudo gigantea* of Schweigger’ in comparing the fossil *Testudo grandidieri* Vaillant, 1885 from Madagascar with the living tortoise on Aldabra Atoll.

10. *T. gigantea* was explicitly associated with Aldabra Atoll by Hubrecht (1881, pp. 43–44) and Boulenger (1889, p. 168), and this geographic association has continued for over a century, until today. Hence, the name *T. gigantea* is firmly attached to Aldabra Atoll.

11. *T. gigantea* Schweigger, 1812 has been consistently recognised as the oldest available name for the Aldabra tortoise for more than 50 years (Rothschild, 1897, p. 407; Williams, 1952, p. 557, footnote 1; see also Bour, 1984a, pp. 162, 169; 2006, p. 15), appearing repeatedly as the senior synonym in authoritative taxonomic reviews of the TESTUDINIDAE (Siebenrock, 1909, p. 529–530; Mertens & Wermuth, 1955, pp. 377–378; Loveridge & Williams, 1957, p. 225; Wermuth & Mertens, 1961, p. 204; 1977, p. 84; Fritz & Havaš, 2006, p. 122; 2007, pp. 265–266).

12. Between 1915 and 2006 various authors stated that Schweigger’s unique specimen of *T. gigantea* was lost or, as was the custom at that time, never designated as a holotype (Rothschild, 1915, p. 430; Bour, 1984a, p. 162; 2006, p. 13; Crumly, 1986, pp. 238–239; Pritchard, 1986, p. 522).

13. Loveridge & Williams (1957, pp. 220 and following pages) reorganised *Testudo*, the land tortoises, into seven genera, with the large, and giant, tortoises assigned to the genus *Geochelone* Fitzinger, 1835. Recognising that there was no available subgenus for the Aldabra tortoise and its close allies (Williams, 1952, p. 557), they established the subgeneric name *Aldabrachelys*, and designated *Testudo gigantea* Schweigger, 1812 as the type species (Loveridge & Williams, 1957, p. 225).

14. Bour (1982, p. 117) stated that Schweigger’s (1812) description of *Testudo gigantea* applied ‘incontestablement’ (unquestionably) to an extinct Mascarene tortoise, now known as *Geochelone (Cylindraspis) indica* (Schneider, 1783). He concluded that the name *Testudo gigantea* Schweigger, 1812 could not be applied to the Aldabra tortoise, claiming that it was a junior synonym of *Testudo indica* Schneider, 1783 and then decided that *T. elephantina* Duméril & Bibron (1835), was the next available name for the Aldabra tortoise.

15. The lectotype of *T. elephantina* (MNHN 7874), the ‘Aldabra tortoise’, was actually collected around 1830 on Mauritius (‘Ile de France’) (Bour, 1982, p. 117; 1984a, p. 171), a Mascarene island where two sympatric species of *Geochelone (Cylindraspis)*, a completely different genus/subgenus of land tortoise, were endemic, until they were exterminated in the 17th or 18th century (Austin et al., 2002). However, there is no doubt that the lectotype is in agreement with the current population of the Aldabra tortoise. Bour (1982, p. 117) further concluded that because the type species of *Aldabrachelys* is *Testudo gigantea* Schweigger (Loveridge & Williams 1957, p. 225), *Aldabrachelys* is a junior synonym of *Cylindraspis* Fitzinger, 1835, of which *Chelonura indica* (Schneider, 1783) is the type species (Fritz & Havaš, 2007, p. 277). Thus, Bour (1982, p. 117) proposed a new genus *Dipsochelys* for the giant tortoises of the Seychelles-Aldabra-Madagascar region, and later (Bour, 1984a; 1984b; 1994, p. 136) published more extensive arguments along these same lines; in all cases, his conclusions were based solely on interpretations of Schweigger’s original description.

16. Bour (1984a, p. 171 footnote 1; 1984b, p. 282) considered that *Testudo dussumieri* Gray, 1831 was a nomen oblitum, and reiterated his arguments for using the later name *Testudo elephantina* Duméril & Bibron, 1835 for the Aldabra tortoise.

17. Pritchard (1986) agreed with Bour (1982; 1984a) that Schweigger's (1812, pp. 327, 362–363) description of *Testudo gigantea* did not fit the Aldabra tortoise, that the correct name for this tortoise was *T. elephantina* Duméril & Bibron, 1835, and that *T. dussumieri* Gray, 1831 was a nomen oblitum. However, unlike Bour, Pritchard (1986) concluded that Schweigger's (1812) description referred to the South American tortoise *Geochelone (Chelonoidis) denticulata*. Pritchard stated (1986, pp. 532–533) that *Aldabrachelys* is a familiar name for the Aldabra tortoise, having been 'in regular usage' by many authors since it was established and that it might be appropriate to request the Commission to conserve it. He also observed that it would be undesirable to use the name *Testudo dussumieri* Gray, 1831, as it was 'extremely unfamiliar' in relation to the Aldabra tortoise.

18. Crumly (1986, p. 237) concluded that Schweigger's (1812) description was not easy to interpret, and observed that *gigantea* was 'the "established" name' for the Aldabra tortoise. In the interests of nomenclatural stability and universality Crumly advised retention of both *gigantea* and *Aldabrachelys* for the Aldabra tortoise; he quoted Stejneger (1933, p. 133) to explain the logic: 'It is not permissible to substitute one uncertainty for another uncertainty, much less an uncertainty for an established certainty.'

19. King & Burke (1989, p. 70) followed Pritchard (1986), as did Broadley & Howell (1991, p. 8) in their checklist and synoptic keys, and used *Aldabrachelys elephantina* for the Aldabra tortoise. However, numerous specialists in chelonian biology and systematics have continued to use the name *gigantea* for the Aldabra tortoise, employing either *Geochelone* or *Aldabrachelys* as the generic name (e.g. Meylan & Auffenberg, 1986, p. 303; 1987, p. 76; Crumly, 1988, p. 2; Ernst & Barbour, 1989, p. 250; Iverson, 1992, p. 249; Hailey, 2000, p. 185; Meylan & Sterrer, 2000, p. 52; Austin & Arnold, 2001, p. 2515; Díaz-Paniagua et al., 2001, p. 719; Zug et al. 2001, pp. 44 and following pages; Austin et al., 2002, p. 281; 2003, p. 1417; Hailey & Lambert, 2002, pp. 121–125, 130, 133–134, 137; Varela & Bucher, 2002, p. 139; Crumly & Sánchez-Villagra, 2004, p. 136, tab. 2; Furrer et al., 2004, p. 178; Márquez et al., 2004, pp. 99, 107, 109; Danilov, 2005, pp. 403–404; Leuteritz et al., 2005, p. 456; Russell et al., 2005; Kuchling, 2006, pp. 71–72; Meylan, 2006, p. 348; Olson et al., 2006, p. 397; Andreone et al., 2007, p. 318; Fritz & Bininda-Emonds, 2007, pp. 301 and following pages; Fritz & Havaš, 2007, p. 265; Leuteritz & Hofmeyer, 2007, p. 560; Márquez et al., 2007, p. 337; Reynolds et al., 2007, p. 31; Anquetin & Claude, 2008, p. 341; and Chiari et al., 2008, pp. 426–427, 430, 433). This is not to mention more than 100 papers on biology, ecology, husbandry, and other topics that deal with the Aldabra tortoise that were published in the second half of the 20th century (Stimson [in Pritchard, 1986, p. 522] reported 139 publications in 'the last 50 years' (i.e., before 1986); see also Frazier, 2006a). A preliminary review indicates more than 100 papers published during just nine years between 2000 and 2008 that use *gigantea* as the species name for the Aldabra tortoise (a list of these references has been deposited with the Commission Secretariat); these publications are primarily in peer-reviewed journals and academic books, but also in reports of intergovernmental organisations as well as books for general readership, and they deal with a variety of

disciplines including conservation, ecology, evolutionary theory, gerontology, husbandry, parasitology, physiology, wildlife management, and veterinary medicine (e.g. for just the year 2008 these include: Bays et al., 2008, p. 144; Burgin & Renshaw, 2008, p. 61; Eisenhauer, 2008, pp. 209, 213; Gaalema & Benboe, 2008; Gabrisch et al., 2008, p. 655; Goldsmith, 2008, p. 14; Hansen et al., 2008, pp. 3, 11; Kraus, 2008, p. 316; Leonardi et al., 2008, p. 7; Lutfullah et al., 2008, pp. 141–145, 147–149; Nardoni et al., 2008, p. 164; and O'Malley, 2008, p. 53). Likewise, major international organisations that depend on clear and stable zoological nomenclature continue to use *gigantea* for the Aldabra tortoise: the United Nations Environmental Programme-World Conservation Monitoring Centre (UNEP-WCMC, 2008); the Convention on International Trade in Endangered Species of Wild Fauna and Flora (CITES) (Charette et al., 1999; Fritz & Havaš, 2006, p. 122); the European Commission on Integrated Tariff (EC, 2002, p. 3256); the European Food Safety Authority (EFSA, 2007, p. 18); the International Union for the Conservation of Nature (Swingland & Klemens, 1989; IUCN, 2008); the Integrated Taxonomic Information System (ITIS, 2008); the International Species Information System (ISIS, 2008); and the EMYSystem (2008). Moreover, government legislation and reports of the Republic of Seychelles, the country where the Aldabra tortoise is found, continue to refer to *Geochelone gigantea* (e.g. GEF, 1992, p. 3; Republic of Seychelles, 1999, p. 121; Ferguson and Carolus, 2005, p. [10]), as does national legislation from other countries, such as Australia (e.g. New South Wales, 2005, p. 5149). These diverse examples clearly demonstrate established usage of the name *Testudo gigantea* Schweigger, 1812 for the Aldabra tortoise.

20. Gerlach & Canning (1995, pp. 133 and following pages) proposed *Dipsochelys resurrecta*, a nomen nudum, without description or type material. Gerlach subsequently abandoned this name and stated that *T. dussumieri* Gray, 1831 was the correct name for the Aldabra tortoise (e.g. Gerlach, 1997, p. 28; 2001, pp. 2 and following pages; 2004a, p. 67 and following pages; 2004b, p. 10; see Frazier, 2006b, pp. 369 and following pages as well as Fritz & Havaš, 2007, pp. 266–267 for more details).

21. Because Gray (1831a, p. 3) published *Testudo dussumieri* as a nomen nudum and then (1831b, p. 9) only as a junior synonym of *Testudo indica* Schneider, 1783 (Fritz & Havaš, 2007, p. 270), his name would ordinarily be unavailable under Article 11.6 of the Code. However, Bour (2006, p. 21) recently argued that the listing of '*T. Dussumieri*. Schlegel.' by Fitzinger (1835, p. 122) made the name *Testudo dussumieri* Gray, 1831 available under Article 11.6.1 of the Code. Reversing his previous position (Bour, 1984a, p. 171 footnote 1; 1984b, p. 282) that this was a nomen oblitum, Bour (2006, p. 21) stated that *T. dussumieri* Gray, 1831 was the correct name for the Aldabra tortoise, and designated a lectotype in the Leiden museum (RMNH 3231), from the type series listed by Gray (1831b, p. 9).

22. In addition to the considerable taxonomic and nomenclatural confusion that surrounded the giant tortoise of Aldabra Atoll during the 19th and early 20th century, since the publication of Bour's (1982) paper there has been noteworthy nomenclatural instability and uncertainty (e.g. see citations in Frazer, 2006a and Fritz & Havaš, 2007, pp. 265–267). During just the past two decades no fewer than five generic names (*Aldabrachelys*, *Dipsochelys*, *Geochelone*, *Megalochelys*, and *Testudo*) and three specific names (*dussumieri*, *elephantina* and *gigantea*), in no fewer than eight binominal combinations, have been used explicitly to refer to the giant

tortoise that lives on Aldabra Atoll. Moreover, recent studies (e.g. Austin et al. 2003; Palkovacs et al., 2003; Karanth et al., 2005) indicate that six other nominal species, three of which are in contemporary use (Rhodin et al., 2008, p. 000.12), are also synonyms of *T. gigantea*: *Testudo daudinii* Duméril & Bibron, 1835; *Testudo hololissa* Günther, 1877; *Testudo ponderosa* Günther, 1877; *Testudo sumeirei* Sauzier, 1892; *Testudo gouffei* Rothschild, 1906; and *Dipsochelys arnoldi* Bour, 1982. Hence, some authors feel that, not counting erroneous spellings and nomina nuda, no fewer than nine species names have been applied to the Aldabra tortoise during the past two decades (e.g. Fritz & Havaš, 2007, pp. 265–267).

23. Proponents of replacing *Testudo gigantea* Schweigger, 1812 with either *Testudo elephantina* Duméril & Bibron, 1835 or *Testudo dussumieri* Gray, 1831 – based on speculations about Schweigger's original description – have recognised that numerous authors in hundreds of papers in a wide variety of scientific publications have for decades referred to the Aldabra tortoise as *gigantea* (see Stimson in Pritchard, 1986, p. 522; Gerlach, 2001, p. 23, tab. 1). Bour (1984b, p. 281), when asserting that Schweigger's (1812) holotype of *T. gigantea* was really *Cylindraspis indica* (Schneider, 1783), admitted that 'Nomenclatural novelties which arise are such that we are somewhat embarrassed to run counter to an apparently satisfying system'. Later, when declaring that the same specimen (the holotype of *T. gigantea*) was '*Chelonoidis denticulata* (Linnaeus, 1766)', Bour (2006, p. 15) wrote that '... from the beginning of the 20th century, the valid name for the Aldabra tortoise seemed to have been definitely settled, and the binomina [sic] *Testudo gigantea* or *Geochelone gigantea*, with Schweigger as the author, have been widely used until today'. Gerlach has repeatedly reported that *Geochelone gigantea* is more frequently used than his favoured *Dipsochelys dussumieri* (e.g. Gerlach, 1999a, p. 496; 2001, p. 23, tab. 1; Gerlach & Canning, 1995, p. 133). Pritchard (1986, p. 531) stated that '... *gigantea* has indeed been the name in commonest use for the Aldabra tortoises in the 20th century ...' and 'Of course, invalidation of the familiar epithet *gigantea* represents a rather profound upheaval'. Hence, the proponents of name change have explicitly recognised that their actions entail replacement of the most frequently applied name, rejection of an established system, and nomenclatural 'upheaval' (see also Bour, 1984a, p. 162; Crumly, 1988; Austin et al., 2003, p. 1417; Frazier, 2006a). Indeed, publications that refer to the Aldabra tortoise as *dussumieri* have routinely clarified that this taxon is also known by the specific name *gigantea* (either *Aldabrachelys gigantea* or *Geochelone gigantea*), for the authors realise that the *gigantea* is the accustomed name and neither *dussumieri* nor *elephantina* are widely recognised (e.g. Gerlach & Canning, 1998a, p. 3; 1998b, p. 133; Gerlach, 1999a, p. 496; 1999b, p. 34; 2004b, p. 10; 2005, p. 937; Palkovacs et al., 2002, p. 216; 2003, p. 1403; Jacobson, 2007, p. 597; Kalandadze & Shapovalov, 2007; Leuteritz et al., [2008]). Bour et al. (2007, p. 105) even felt the need not only to explain that on other occasions *D. dussumieri* is named *Geochelone* (*Aldabrachelys*) *gigantea*, but also to specifically state that a specimen of *D. dussumieri* is not *Cylindraspis vosmaeri* – evidently an attempt to reduce nomenclatural confusion that could have arisen from Bour's previous assertions that *T. gigantea* is a junior synonym of *Cylindraspis indica*, a taxon closely aligned to *C. vosmaeri*. Not only do some authors propose abandoning the established nomenclatural system, but the changes that they propose have been contradictory and unstable. Between 1982 and 1994, Bour (1982; 1984a; 1984b; 1994)

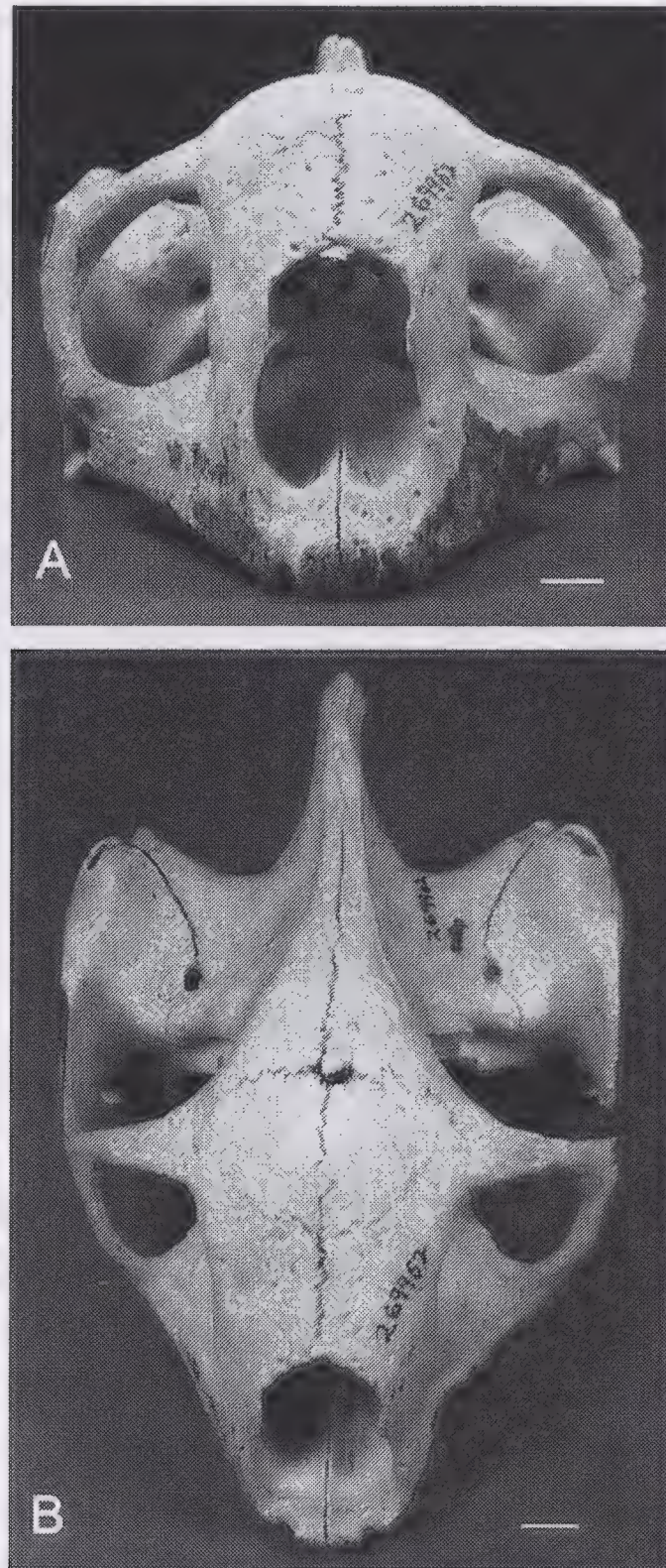


Fig. 1. Skull of neotype of *Testudo gigantea* Schweigger, 1812 (USNM 269962) (photos by Steve Gotte and George Zug): A. Anterior view; scale bar = 8 mm; B. Dorsal view; scale bar = 8 mm.

argued vigorously for using *Dipsochelys elephantina* for the Aldabra tortoise. In 2003, in an international peer-reviewed journal, he used *Aldabrachelys gigantea* (see Austin et al., 2003), but in the same year (Gerlach & Bour, 2003) he used *Dipsochelys dussumieri*, the name which he has been championing most recently (2006; 2008, p. 14).

24. In order to stabilise the nomenclature of the Aldabra tortoise, Frazier (2006a, p. 278) designated a neotype for *Testudo gigantea* Schweigger, 1812 under Articles 72.2 and 75 of the Code, an action developed after extended consultation with numerous specialists in chelonian systematics. The designated neotype is specimen USNM 269962 (an adult male from Dune Patates, South Island, Aldabra Atoll, Republic of Seychelles) housed in the National Museum of Natural History, Smithsonian Institution (see Figs. 1 & 2).

25. In response, Bour (2006) claimed to have rediscovered the long-lost holotype of *Testudo gigantea* Schweigger, 1812 (specimen MNHN 9554 in the Muséum National d'Histoire Naturelle, Paris). This specimen is an 'old stuffed male' recently identified by Bour (2006, pp. 16 figs. 2, 18, tab. 2) as '*Chelonoidis denticulata* (Linnaeus, 1766)'. The only data that are specifically associated with MNHN 9554 are in a hand-written catalogue entry, thought to date from about 1864: registration number '120,' locality

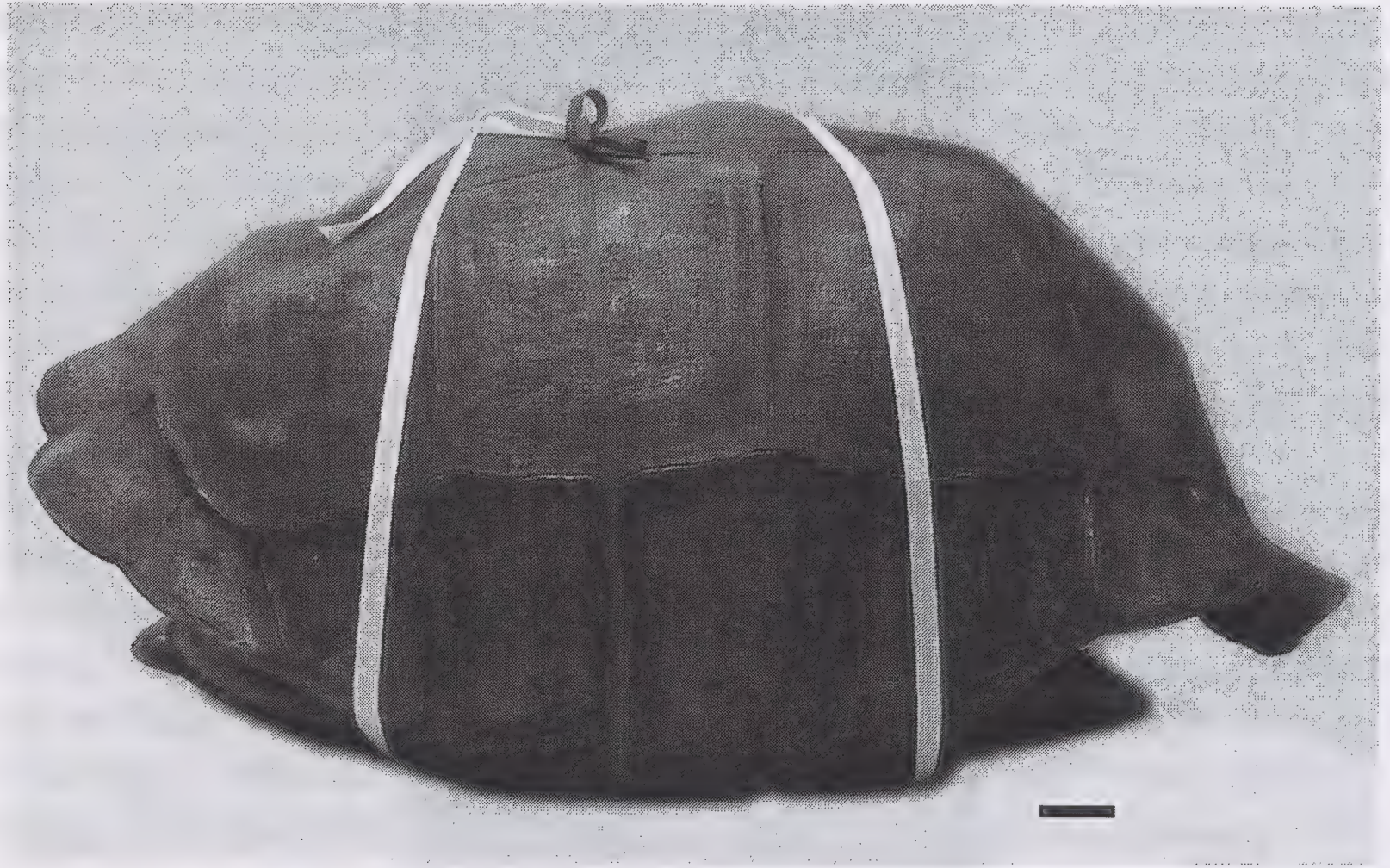


Fig. 2. Lateral view of the left side of the carapace of neotype of *Testudo gigantea* Schweigger, 1812 (USNM 269962); scale bar = 45 mm (photo by Steve Gotte and George Zug).

‘Brésil,’ and catalogued as ‘*Testudo carbonaria*, Dum., Bib., très vieux sujet d’origine inconnue’ (very old specimen of unknown origin) (Bour, 2006, p. 19).

26. The reported rediscovery of Schweigger’s type in the Muséum National d’Histoire Naturelle, Paris, is based on several fundamental but unsubstantiated assertions. Among other things, it assumes that both Schweigger (1812) and Duméril & Bibron (1835) misidentified the holotype of *T. gigantea* (e.g. see Bour, 2008, p. 14), although these authors differentiated between the species with which they made the purported misidentifications (*T. carbonaria*, *T. denticulata*, and *T. gigantea*). Hence, with information available, there will be continued and irresolvable claims and counter claims about the validity of MNHN 9554 as the holotype of *T. gigantea* Schweigger, 1812.

27. If specimen MNHN 9554 were accepted as the long-lost holotype of *T. gigantea* this would once again threaten the use of a specific name that has been in continual usage for over a century and clearly become established in a diverse literature, replacing it with a specific name that was until recently recognised unquestionably as a nomen oblitum, thereby further confounding a decades-long debate that has resulted in nomenclatural instability.

28. The use of the generic name *Dipsochelys* and the specific name *D. dussumieri* (Gray, 1831) for the Aldabra tortoise will continue to cause major confusion. These actions challenge the established nomenclatural system, particularly because of the recent promulgation of these unaccustomed names in the privately published, popular, and promotional literature (e.g. Gerlach, 1997; 2001; 2004a; Vetter, 2002, pp. 42–43; Gerlach & Bour, 2003; Bonin et al., 2006, pp. 209, 220; Bour 2006; Franklin, 2007, pp. 122, 129; Leuteritz et al., [2008]; Pedrono, 2008, pp. 37–39; see also bibliography of Gerlach in Frazier, 2006b). As a result of the present nomenclatural confusion, the Turtle Taxonomy Working Group (2007, pp. 177, 183) recently concluded that there is no clarity about what generic or specific name to use

for the Aldabra tortoise, listing ‘*Aldabrachelys* or *Dipsochelys* [formerly in *Geochelone*]’ and ‘... Aldabran tortoises (*dussumieri* or *gigantea*) ...’; similarly, Iverson et al. (2007, p. 94) and Rhodin et al. (2008, pp. 000.12, 000.22) came to the same confused conclusion. In this light, some authors have used ‘*Aldabrachelys elephantopus*’ [sic], ‘*Dipsochelys* complex’, and ‘*Geochelone* complex’ (Claude & Tong, 2004, pp. 19, 20, 33, 36), or ‘*Geochelone gigantea*’ and ‘*Dipsochelys elephantina*’ (Miller & Dinkelacker, 2007, pp. 232, 242, 249, 260) in the same publication (even on the same page!), applying the different names interchangeably for the same taxon. Other authors have referred to ‘*Dipsochelys gigantea*’ (Gerlach & Canning, 1995, p. 133; Depecker et al., 2006a, p. 511; 2006b, pp. 36–37), oblivious of the fact that *Geochelone* Fitzinger, 1835 is senior to *Dipsochelys* Bour, 1982, and the use of *gigantea* Schweigger for the Aldabra tortoise renders *Dipsochelys* a junior synonym of *Aldabrachelys* Loveridge & Williams, 1957, because the type species of *Aldabrachelys* is *T. gigantea*. Other manifestations of nomenclatural confusion are shown in phylograms that present *Dipsochelys* as distinct from *Geochelone*, where the latter includes *Geochelone gigantea* (Hoffman & Storz, 2007, supplementary figure). These examples from anatomical, conservation, ecological, evolutionary, and paleontological studies further illustrate the levels of nomenclatural confusion that have been generated around this taxon. In this context, Iverson et al. (2007, p. 96) warned ‘... for the sake of nomenclatural stability, we recommend restraint in proposing taxonomic changes until taxon and character sampling are adequate to provide robust support for such changes. To do otherwise will add confusion to an already complex literature (e.g. Frazier, 2006 and Bour, 2006), and may even hamper conservation efforts for this unique and imperilled clade of vertebrates (TTWG, 2007a)’. Hence, *T. gigantea*, the specific name that has been in constant use for more than 100 years to refer to the Aldabra tortoise, used in hundreds of publications by scores of authors, and referred to as the senior synonym in numerous authoritative reviews of the group, may be regarded as the established name for the Aldabra tortoise. It is, therefore, proposed that the names *Aldabrachelys* Loveridge & Williams, 1957, and *Testudo gigantea* Schweigger, 1812 be conserved under Article 75.8 of the Code, by retention of the neotype of *Testudo gigantea* Schweigger, 1812 as the name-bearing type and by suppression of the name *T. dussumieri* Gray, 1831.

29. The International Commission on Zoological Nomenclature is accordingly asked:

(1) to use its plenary power:

- (a) to set aside all previous type fixations for the nominal species *Testudo gigantea* Schweigger, 1812 and retain neotype USNM 269962 in the National Museum of Natural History, Smithsonian Institution, as designated and described by Frazier (2006a), as the name-bearing type;
- (b) to suppress the name *dussumieri* Gray, 1831, as published in the binomen *Testudo dussumieri*, for the purposes of the Principle of Priority but not for those of the Principle of Homonymy;

(2) to place on the Official List of Generic Names in Zoology the name *Aldabrachelys* Loveridge & Williams, 1957, type species by original designation *Testudo gigantea* Schweigger, 1812;

(3) to place on the Official List of Specific Names in Zoology the name *gigantea*, Schweigger, 1812, as published in the binomen *Testudo gigantea* and as defined

by the neotype designated in (1)(a) above, the specific name of the type species of *Aldabrachelys* Loveridge & Williams, 1957;

- (4) to place on the Official Index of Rejected and Invalid Specific Names in Zoology the name *dussumieri* Gray, 1831, as published in the binomen *Testudo dussumieri* and as suppressed in (1)(b) above.

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Case 3472***Cetiosaurus* Owen, 1841 (Dinosauria, Sauropoda): proposed conservation of usage by designation of *Cetiosaurus oxoniensis* Phillips, 1871 as the type species**

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Abstract. The purpose of this application, under Article 81.1 of the Code, is to preserve stability in the taxonomy of sauropod dinosaurs by designating *Cetiosaurus oxoniensis* as the type species of *Cetiosaurus*. The genus *Cetiosaurus* (including the species *C. medius* and *C. oxoniensis*) was established during the earliest period of research on sauropod dinosaurs, and is historically significant. The name *Cetiosaurus* was fixed to the type species *Cetiosaurus medius*, a sauropod of indeterminate affinities; however, the fragmentary nature of the type material of *C. medius*, combined with the subsequent description of much more complete Middle Jurassic sauropod material as *Cetiosaurus oxoniensis*, has meant that subsequent literature has overwhelmingly adopted *C. oxoniensis* over *C. medius* as the primary exemplar of *Cetiosaurus*. Stability would be best served by designating *Cetiosaurus oxoniensis* as the type species of the genus *Cetiosaurus* in place of the current type species, *C. medius*.

Keywords. Nomenclature; taxonomy; Dinosauria; Sauropoda; CETIOSAURIDAE; *Cetiosaurus*; *Cetiosaurus oxoniensis*; *Cetiosaurus medius*; England; Europe; Middle Jurassic.

1. The generic name *Cetiosaurus* was first published by Owen (1841, p. 457) without any associated specific name. It was based primarily on material found by John Kingdon in 1825, but no specimen numbers were given. *Cetiosaurus* was among the first named sauropod dinosaurs and, as a result, has become a 'wastebasket' taxon, with much material indiscriminately referred to it. As detailed by Upchurch & Martin (2003, p. 208), the stratigraphic range of *Cetiosaurus*, if all referrals were supported, would extend from the Bajocian (Middle Jurassic) to the Barremian (Early Cretaceous), a range of about 45 million years.

2. The first published species of *Cetiosaurus* were *C. hypoolithicus* Owen, 1842 and *C. epioolithicus* Owen, 1842, both published in the same report (Owen, 1842a, p. 12). The type material was not specified for either species, and neither was illustrated or diagnosed; therefore both species are nomina nuda, and are ineligible for fixation as the type species (Upchurch & Martin, 2003, p. 209).

3. The next published species of *Cetiosaurus* were *C. brevis*, *C. brachyurus*, *C. medius* and *C. longus*, all described in a single report by Owen (1842b, pp. 94, 100a, 100b, 101). Since descriptions of all four species were furnished, they are not nomina nuda and are thus available names for fixation as the type species. Although Owen did not explicitly designate any of these species as the type, he did note of the *C. medius* material that 'it is principally on these bones, with others subsequently discovered and in the collection of Mr. Kingdon, that the characters of the *Cetiosaurus* were first determined' (Owen, 1842b, pp. 100–101). *C. medius* is thus the type species of *Cetiosaurus* under Article 69.1.1 of the Code (Type species by subsequent designation), an interpretation endorsed by, for example, Steel (1970, p. 64).

4. *Cetiosaurus oxoniensis* was described by Phillips (1871, p. 291) from a large series of remains from the Forest Marble (Bathonian, Middle Jurassic of Oxfordshire), among which no type specimen was formally designated. Phillips (1871, pp. 290–291) wrote only 'I propose for the species found in the immediate vicinity of Oxford and elsewhere, the only one for which sufficient materials are collected to serve for determining its characters, the title Oxoniensis', so that Phillips's specimens found in this area form a syntype series. Among these specimens, the largest of three individuals from Bletchingdon Station, near Enslow Bridge, is the most complete and diagnostic; it was therefore designated as the lectotype (OUMNH J13605–13613, J13615–16, J13619–J13688, J13899 in the Oxford University Museum of Natural History) by Upchurch & Martin (2003, p. 216). The material represents a non-neosauropod eusauropod. It is important due to its historical significance as the first sauropod known from adequate remains, illustrating the basic sauropod body plan, and also because of the light it casts on the evolution of sauropods, being one of the most derived taxa outside the clade Neosauropoda (Upchurch, 1998, fig. 19; Upchurch et al., 2004, fig. 13.18). *Cetiosaurus* is the type genus of the family CETIOSAURIDAE Lydekker, 1888 and was used as a specifier in the phylogenetic definition of the clade CETIOSAURIDAE (Upchurch et al., 2004, p. 301).

5. Several further species of *Cetiosaurus* have also been erected. As summarised by Upchurch & Martin (2003, p. 215), of the thirteen named species, three are nomina nuda, two are junior objective synonyms, four are nomina dubia, and four are diagnosable taxa (*C. brevis*, *C. oxoniensis*, *C. glymptonensis* Phillips, 1871, p. 291 and *C. humerocristatus* Hulke, 1874, p. 17). These last four cannot be congeneric as they represent several different sauropod groups.

6. Under a strict application of the Code, *Cetiosaurus medius* is the type species of *Cetiosaurus*. However, the name *Cetiosaurus* has invariably been associated with the species *C. oxoniensis*, and specifically the Bletchingdon Station material (e.g. Owen, 1875; Hatcher, 1903; Huene, 1904, 1927; Fraas, 1908; Janensch, 1914, 1929; Matthew, 1915; Coombs, 1975; Wild, 1978; Bonaparte, 1986, 1999; Martin, 1987; Upchurch, 1998; Casanovas et al., 2001; Upchurch & Martin, 2002, 2003; Liston, 2004; Day et al., 2004; Upchurch et al., 2004; Sánchez-Hernández, 2005; Wedel, 2005,

2007; Yates, 2006, 2007; Galton & Knoll, 2006; Moser et al., 2006; Naish & Martill, 2007; Taylor & Naish, 2007). The stability of use of this species in the literature as representing *Cetiosaurus* is indicated by the fact that no other generic name has ever been proposed for *C. oxoniensis*; nor has it ever been referred to as '*Cetiosaurus*' *oxoniensis*. Enforcing the strict application of the Code would lead to considerable nomenclatural confusion and inconsistency.

6. For this reason, Upchurch & Martin (2003, p. 215) informally treated *Cetiosaurus oxoniensis* as the type species of *Cetiosaurus* pending a promised petition to the Commission.

7. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to set aside all previous fixations of type species for the nominal genus *Cetiosaurus* Owen, 1841 and to designate *Cetiosaurus oxoniensis* Phillips, 1871 as the type species;
- (2) to place on the Official List of Generic Names in Zoology the name *Cetiosaurus* Owen, 1841 (gender: masculine), type species *Cetiosaurus oxoniensis* Phillips, 1871, as ruled in (1) above;
- (3) to place on the Official List of Specific Names in Zoology the name *oxoniensis* Phillips, 1871, as published in the binomen *Cetiosaurus oxoniensis* (specific name of the type species of *Cetiosaurus* Owen, 1841), as ruled in (1) above.

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Case 3474***Aplonis* Gould, 1836 (Aves: STURNIDAE): proposed conservation of spelling**

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Abstract. The purpose of this application, under Articles 23.9.3 and 81.1 of the Code, is to conserve the long- and widely-accepted spelling *Aplonis* Gould, 18 October 1836 for the Indo-Australasian glossy starlings (STURNIDAE) by suppression of the prior but little used spelling *Aplornis* Gould, 1 or 3 October 1836.

Keywords. Nomenclature; taxonomy; Aves; STURNIDAE; *Aplonis*; *Aplornis*; *Aplonis marginata*; glossy starlings; Indo-Australasia.

1. The 24 or so species of west Pacific glossy starlings (Aves: STURNIDAE) have been known almost universally by the generic name *Aplonis* since the genus was described by John Gould in 1836. Gould (1836a, p. 73) read *Aplonis* for publication at the 26 July 1836 meeting of the Zoological Society of London, and it was published in that spelling in the number of the Society's *Proceedings* which appeared on 18 October 1836 (Sclater, 1893). Described with it were two new named species, *Aplonis marginata* Gould, 1836 and *A. fusca* Gould, 1836. The former (a junior synonym of *Lanius tabuensis* Gmelin, 1788, a name overlooked by Gould) was subsequently chosen as the type species by G.R. Gray (1855, p. 66), who there corrected previous errors in designation. Earlier, he (Gray 1840, p. 40; 1841, p. 53) had designated two type species together in *Aplonis*, namely '*A. novae hollandiae*, (Lath. / Gm.)' = *Turdus novaehollandiae* Gmelin, 1789 and '*A. fuscus*, Gould?' = *Aplonis fusca* Gould, 1836. The first of these nominal species was not included in Gould's description of the genus, and the second, though included, had been cited by Gray (1840, p. 40; 1841, p. 53) under a question mark. Thus neither designation is admissible under Articles 67.2 and 67.5.3 of the Code, contra Mathews (1930, p. 846) and Amadon (1962, p. 75).

2. *Aplonis* in that spelling has since been used for the west Pacific glossy starlings in hundreds of references in scientific journals, conservation reports, ornithological newsletters, regional and global checklists, regional and global handbooks, and field guides up to the present day. In the national Australian ornithological journal, *Emu*, it was used in 98 papers alone during the 20th century, covering diverse subjects. Reference works using *Aplonis* include, among many others:

(1) global checklists: Sharpe, 1909; Amadon, 1962; Wolters, 1980; Sibley & Monroe, 1990; Monroe & Sibley, 1993; Wells, 1998; Dickinson, 2003; Gill & Wright, 2006; Clements, 2007;

(2) regional checklists: R.A.O.U. Checklist Committee, 1926 (Australia); Mathews, 1930 (Australasian region); Chasen, 1935 (Malaysia); Mayr, 1941 (New Guinea); Ripley, 1961 (Indian subcontinent); Schodde, 1975 (Australia); Beehler & Finch, 1985 (New Guinea); White & Bruce, 1986 (east Indonesia); van Marle & Voous, 1988 (Sumatra); Christidis & Boles, 1994 (Australia); Inskipp et al., 1996 (Oriental region); Clayton et al., 2006 (Australia);

(3) global handbooks and conservation manuals: Sharpe, 1890; Mayr, 1942, p. 37; Greenway, 1967; Collar & Andrew, 1988, pp. 164–165; Sibley & Ahlquist, 1990, pp. 636, 864; Feare & Craig, 1998;

(4) regional handbooks and conservation manuals: Mathews, 1925–1927, 1928 (Australia, Norfolk and Lord Howe Islands); Smythies, 1953 (Myanmar); Rand & Gilliard, 1967 (New Guinea); duPont, 1971 (Philippines); Ali & Ripley, 1972 (India); Diamond, 1972 (New Guinea); duPont, 1976 (south Pacific); Ovington, 1978 (Australia and territories); Hannecart & Letocart, 1980 (New Caledonia); Watling, 1982 (Fiji, Tonga, Samoa); Schodde et al., 1983 (Norfolk Island); Clunie, 1984 (Fiji); Schodde & Tiedemann, 1986 (Australia); Gonzales & Rees, 1988 (Philippines); Bregulla, 1992 (Vanuatu); Schodde & Mason, 1999 (Australia); Smythies, 1999 (Borneo); Garnett & Crowley, 2000 (Australia); Mayr & Diamond, 2001 (Melanesia); Barrett et al., 2003 (Australia); Hadden, 2004 (Solomons); Beruldsen, 2003 (Australia); Peter, 2006 (Australia); Steadman, 2006 (tropical west Pacific); Wells, 2007 (Malaysia-Thailand);

(5) field guides: Mayr, 1945 (Melanesia and Polynesia); Delacour & Mayr, 1946 (Philippines); Delacour, 1947 (Malaysia); Glenister, 1951 (peninsular Malaysia); Delacour, 1966 (New Caledonia); Wildash, 1968 (Vietnam); Lekagul & Cronin, 1974 (Thailand); King & Dickinson, 1975 (southeast Asia); Beehler et al., 1986 (New Guinea); Pratt et al., 1987 (Polynesia, Micronesia); McKinnon, 1988 (Java, Bali); Coates & Bishop, 1997 (east Indonesia); McKinnon & Phillipps, 1993 (Greater Sundas); Pizzey & Knight, 1997 (Australia); Holmes & Phillips, 1998 (peninsular Malaysia); Doughty et al., 1999 (Solomons, Vanuatu, New Caledonia); Simpson & Day, 1999 (Australia); Robson, 2000 (southeast Asia); Coates & Peckover, 2001 (east Papuasia); Watling, 2001 (western Polynesia); Morecombe, 2003 (Australia).

3. As shown by Mathews (1938, p. 342; 1945, p. 319), the name had actually been published available two weeks earlier, and in a different spelling, *Aplornis*. On 1 or 3 October 1836, it appeared under Gould's authority with a skimmed description and without named species in a newspaper report of the meeting of the Zoological Society of London at which Gould had read *Aplonis* for publication (Gould, 1836b, p. 152). Gould is the author of the name under Article 50.2 of the Code. It is one of 10 generic and 42 specific names which, long-accepted as having been published by Gould and others in the *Proceedings of the Zoological Society of London* and diverse monographs, have since been found to have been published prematurely but available in prior, brief newspaper reports of the meetings of the Zoological Society at which the names were first read (Bruce & McAllan, 1990). Where senior, they are valid, as is the case with *Aplornis*. Schodde & Bock (1997) subsequently applied to the International Commission on Zoological Nomenclature for blanket suppression of

the ‘newspaper’ names. Although the Commission rejected Schodde & Bock’s (1997) application in Opinion 2026 (BZN 2003 60: 79–80), a number of Commissioners recommended case by case submission for those names that would create instability if adopted. *Aplornis* Gould versus the long-established spelling *Aplonis* is one such case.

4. It might be thought that Gould’s intention was to spell the name ‘*Aplornis*’; but this does not seem to be the case. Not only did he spell it *Aplonis* three times without variation in his description of the genus in the *Proceedings of the Zoological Society of London* (Gould 1836a, p. 73), but in the only other publication in which he used the name – the *Supplement* to his *The Birds of Australia* (Gould 1869, text and title to plate 33) – he again used *Aplonis* exclusively, both as a generic name and as an anglicised vernacular, ‘Shining *Aplonis*’. Gould never explained the source of the name. In one interpretation since, David & Gosselin (2002, p. 270) considered *Aplonis* to be an arbitrary combination of letters derived from the Greek *aploos* = simple and *ornis* = bird, and implicitly correct if original (Articles 11.3, 32.2, 32.5 of the Code). In another admitted guess, the R.A.O.U. Checklist Committee (1926, p. 118) suggested ‘*Apollonis*’, from Apollo, the sun – ‘the bird is a “shining” bird’. Thus it is likely that the original spelling *Aplornis* in the newspaper report (Gould, 1836b, p. 152) reflects editorial emendation. Gould never made any mention of that spelling and, in common with other 19th century contemporaries, was almost certainly unaware of it.

5. Until Mathews (1938) unearthed the prior spelling *Aplornis*, *Aplonis* had been used in that spelling throughout all 19th and early 20th century literature dealing with the west Pacific glossy starlings (e.g. Gray, 1840, 1855; Sharpe in Gould, 1875–1888; Sharpe, 1890). Since then, *Aplonis* has been kept overwhelmingly in prevailing use as well, as demonstrated in paragraph 2 above. In contrast, the earlier spelling *Aplornis* has been widely ignored, and Wolters (1975–82, p. 437) and McAllan & Bruce (1989, pp. 73–74) explicitly rejected it, faulty reasoning notwithstanding. Up to the present, we have found just eight references that have accepted *Aplornis* as valid: Taka-Tsukasa & Yamashina (1931), Mathews (1938, 1945, 1946), Hachisuka et al., (1942), Bruce & McAllan (1990), McAllan (2007) and Christidis & Boles (2008, pp. 39, 208). Of these, only three are from the last 50 years, and four are in little known papers or works; in Mathews (1946, p. 145), moreover, *Aplornis* and *Aplonis* are both used – and confused – as valid.

6. Potential adoption of the senior spelling *Aplornis* will bring uncertainty about the correct name, with consequent instability. That the difference between *Aplonis* and *Aplornis* is in only one letter is a central part of the problem. It can be expected to engender confusion and misspelling, and will be sufficient to hinder, for example, digital searches for information on the glossy starlings by novice students and others aware of only one of the spellings. In the immediate future, this will be particularly serious for searches employing the so far little-used *Aplornis*.

7. Because the species of *Aplonis* occur throughout the west Pacific, west to southeast Asia and northeast India, north to Micronesia, east to Polynesia and south to Indonesia, New Guinea and Australia (Dickinson, 2003, pp. 651–653), correction to the spelling of that name will have impacts for a number of countries. Six species occur in New Guinea and its satellite archipelagos, five in Indonesia (excluding New Guinea), seven in the Solomon Islands, another seven in Micronesia and Polynesia

and two in the Philippines; one widespread species reaches Australia and another southeast Asia. A change to *Aplornis* will thus affect usage and nomenclatural stability in the ornithological literature of not only Indonesia, Melanesia and Polynesia but also other South-East Asian countries and Australia.

8. *Aplornis* cannot be set aside as a disused senior synonym under Article 23.9.2 of the Code because it has been used as valid several times in the 20th century (see para. 5 above). Schodde et al. (2007, p. 277) argued instead that *Aplonis* Gould, 1836 was an incorrect subsequent spelling of *Aplornis* Gould, 1836, both names being based by reference on Gould's original reading of the genus before the Zoological Society of London on 26 July 1836. In that case, *Aplonis* may, being in prevailing use, replace *Aplornis* as a correct original spelling under Article 33.3.1. Nevertheless, in adopting the spelling *Aplornis*, Christidis & Boles (2008, p. 208) disregarded such views. Application of Article 33.3.1 here could be open to challenge because the attribution of *Aplonis* Gould to the publication of the original spelling *Aplornis* is indirect, via Gould's reading before the Zoological Society of London in July 1836: see Schodde et al. (2007, pp. 276–277) and Schodde & Bock (2008, p. 43) for the sources and interconnections among such Gould names. Moreover, 'prevailing use', however much it may favour *Aplonis* over *Aplornis*, must be interpreted according to its definition in the Glossary of the Code (Article 89.1), and generalities in that definition could lead to future quibbling as well.

9. We therefore believe that Article 23.9.3 of the Code applies more appropriately to this case. So, to maintain the long- and overwhelmingly-accepted spelling *Aplonis* Gould firmly in its accustomed usage, we propose suppression of the little-known prior spelling, *Aplornis* Gould.

10. The International Commission on Zoological Nomenclature is accordingly asked to use its plenary power:

- (1) to suppress the generic name *Aplornis* Gould, [1 or 3 October] 1836 for the Purposes of the Principle of Priority but not for the Purposes of the Principle of Homonymy;
- (2) to place on the Official List of Generic Names in Zoology the name *Aplonis* Gould, 1836 (gender: feminine), type species by subsequent designation *Aplonis marginata* Gould, 1836;
- (3) to place on the Official List of Specific Names in Zoology the name *marginata* Gould, 1836, as published in the binomen *Aplonis marginata* (specific name of the type species of *Aplonis* Gould, 1836);
- (4) to place on the Official Index of Rejected and Invalid Generic Names in Zoology the name *Aplornis* Gould, [1 or 3 October] 1836, as suppressed in (1) above.

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This application has been evaluated and approved by the Standing Committee on Ornithological Nomenclature (SCON) of the International Ornithological Committee.

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Comments on this case are invited for publication (subject to editing) in the *Bulletin*; they should be sent to the Executive Secretary, I.C.Z.N., c/o Natural History Museum, Cromwell Road, London SW7 5BD, U.K. (e-mail: iczn@nhm.ac.uk).

Case 3431***Procynosuchus* Broom, 1937 (Therapsida, Cynodontia): proposed precedence over *Cyrbasiodon* Broom, 1931 and *Parathrinaxodon* Parrington, 1936**

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Abstract. The purpose of this application, under Articles 23.9.3 and 81.2.3 of the Code, is to conserve the generic name *Procynosuchus* Broom, 1937 for a group of Permian cynodonts (family PROCYNOSUCHIDAE) by giving it precedence over the older names *Cyrbasiodon* Broom, 1931 and *Parathrinaxodon* Broom, 1936 whenever these genera are considered synonymous. These cynodonts are of considerable importance to synapsid phylogeny and represent a key point in widely used examples of transitional series in the fossil record.

Keywords. Nomenclature; taxonomy; Therapsida; Cynodontia; PROCYNOSUCHIDAE; *Procynosuchus*; *Cyrbasiodon*; *Parathrinaxodon*; *Procynosuchus delaharpeae*; *Cyrbasiodon boycei*; *Parathrinaxodon proops*; Permian; Africa; Germany; Russia.

1. Broom (1931, p. 163) established the generic name *Cyrbasiodon* for a genus of cynodonts from the Upper Permian *Dicynodon* Assemblage Zone of South Africa. The type species (by monotypy) is *Cyrbasiodon boycei* Broom, 1931 (p. 163). The type and only known specimen of *Cyrbasiodon boycei* is an isolated maxilla housed in the Durban Museum, Natal, South Africa.

2. Parrington (1936, p. 132) established the name *Parathrinaxodon* for a genus of cynodonts from Upper Permian deposits in the Ruhuhu Valley of Tanzania. The type species (by monotypy) is *Parathrinaxodon proops* Parrington, 1936 (p. 132). The type and only known specimen of *Parathrinaxodon proops* is a partial skull, UMZC T810, housed in the University Museum of Zoology, Cambridge, England.

3. Broom (1937, p. 314) established the generic name *Procynosuchus* for a genus of cynodonts from the Upper Permian *Dicynodon* Assemblage Zone of South Africa. The type species (by monotypy) is *Procynosuchus delaharpeae* Broom, 1937 (p. 314). The holotype of *Procynosuchus delaharpeae* is RC 5, a complete skull housed in the Rubidge Collection, Wellwood, Graaff-Reinet, South Africa. Additional material consists of a nearly complete skeleton from the Luangwa Valley of Zambia (OUMNH TSK 34, housed in the Oxford University Museum of Natural History,

Oxford, England), a partial skull from the Ruhuhu Valley of Tanzania (GPIT K92, housed in the Institut und Museum für Geologie und Paläontologie der Eberhard-Karls-Universität Tübingen, Germany), and numerous skulls from South Africa (e.g. AMNH 8220, housed in the American Museum of Natural History, New York, U.S.A.; BP/1/226, 591, 1545, 1559, 2600, 3758, and 5832, housed in the Bernard Price Institute, University of the Witwatersrand, Johannesburg, South Africa; NMQR 280, housed in the National Museum, Bloemfontein, South Africa; RC 12, 72, 92, and 132, housed in the Rubidge Collection, Wellwood, Graaff-Reinet, South Africa; SAM-PK-K338, K5339, K5819, K7600, K8511, and K10394, housed in Iziko, the South African Museum, Cape Town, South Africa). Also, a partial skull assigned to *Procynosuchus* sp. is known from Permian deposits in Germany (Sues & Boy, 1988).

4. *Cyrbasiodon*, *Parathrinaxodon* and *Procynosuchus* are subjective synonyms (Hopson & Kitching, 1972; Hopson, 1991; Battail, 1991; Abdala & Allinson, 2005; Botha et al., 2007). Because of the extremely fragmentary nature of the holotype of *Cyrbasiodon boycei*, *Cyrbasiodon* has had a chequered taxonomic history. It was originally described by Broom (1931) as a probable primitive cynodont, but was considered by several subsequent authors to be a 'scaloposaurian' therocephalian (e.g. Broom, 1932; Haughton & Brink, 1954; Crompton, 1955; Watson & Romer, 1956; Romer, 1961). Mendrez (1972) redescribed the holotype of *Cyrbasiodon boycei*, assigning it to PROCYNOSUCHIDAE. She noted that it was extremely similar to the procynosuchids of the genera *Leavachia* Broom, 1948 and *Parathrinaxodon* Parrington, 1936, but concluded 'it seems preferable to maintain *Cyrbasiodon* as a separate genus, until the variations of the other PROCYNOSUCHIDAE are better known' (Mendrez, 1972, p. 51). However, additional research on procynosuchids has demonstrated that the features used by Broom (1931, 1942, 1948) to diagnose several taxa in this group, including *Cyrbasiodon*, are not reliable and can instead be interpreted as individual and ontogenetic variation as well as postmortem deformation (Brink & Kitching, 1951; Brink, 1963; Anderson, 1968; Hopson & Kitching, 1972; Battail, 1991). Therefore, many of the nominal procynosuchid taxa, including *Leavachia* Broom, 1948 and *Parathrinaxodon* Parrington, 1936, represent either ontogenetic variants or taphonomically distorted specimens of *Procynosuchus* (Battail, 1991; Hopson, 1991; Abdala & Allinson, 2005). As such, *Cyrbasiodon* has been added to the synonymy of *Procynosuchus* in more recent treatments of the genus (e.g. Battail, 1991). A new species of *Cyrbasiodon*, *C. vladimiriensis*, was established by Tatarinov (2004) on the basis of a maxillary fragment with two postcanine teeth (PIN 4818/35, housed in the Paleontological Institute of the Russian Academy of Sciences, Moscow, Russia) from the Upper Permian of the Vladimir Region of Russia. However, the diagnostic features for this taxon given in Tatarinov's (2004) description are not reliable, and it is, in our opinion, best regarded as *Procynosuchus* sp.

5. *Procynosuchus* has been universally utilised as a valid genus name in palaeontological literature since its description and represents one of the most frequently referenced cynodont taxa. A list of 75 publications (25 of which were published in the last eight years) that use *Procynosuchus* as the valid name for this taxon has been submitted to the Secretariat. In addition to its omnipresence in Permo-Triassic therapsid technical literature (see Hopson & Kitching, 2001; Rubidge & Sidor, 2001), *Procynosuchus* has been extensively referenced in popular literature on evolutionary

transitions in the fossil record (e.g. Hopson, 1987; Gould, 1993; Martin, 2004) and textbooks (e.g. Romer, 1966; Ridley, 1993; Kardong, 2002; Pough et al., 2002). Furthermore, original and cast material labeled as *Procynosuchus* is featured in many museum exhibits on mammal evolution and Permo-Triassic vertebrates including, but not limited to exhibits in the Fukui Prefectural Dinosaur Museum, Katsuyama, Japan; The Museum Korbach, Korbach, Germany; the Institut und Museum für Geologie und Paläontologie der Eberhard-Karls-Universität Tübingen, Tübingen, Germany; The Kitching Gallery of the Bernard Price Institute, University of the Witwatersrand, Johannesburg, South Africa; The National Museum, Bloemfontein, South Africa; Iziko, The South African Museum, Cape Town, South Africa; The University Museum of Zoology, Cambridge, England; and the Oxford University Museum of Natural History, Oxford, England. Conversely, the genera *Cyrbasiodon* and *Parathrinaxodon* are known by only a small group of non-mammalian cynodont taxonomists.

6. The skull and postcranium of *Procynosuchus delaharpeae* have been thoroughly described by Kemp (1979, 1980) on the basis of a nearly complete skeleton (OUMNH TSK 34) from Zambia. Because it is known from extensive, well-described material, *Procynosuchus* has been used as an operational taxonomic unit in many analyses of therapsid (especially cynodont) phylogeny (e.g. Hopson & Barghusen, 1986; Battail, 1991; Rowe, 1993; Hopson & Kitching, 2001; Sidor & Smith, 2004; Botha et al., 2007; Abdala, 2007). *Procynosuchus* holds additional importance as the type genus of the family PROCYNOSUCHIDAE, currently in wide use in therapsid literature (e.g. Hopson & Barghusen, 1986; Brink, 1986; Battail, 1991; Tatarinov, 2004, 2005). It is in the best interests of nomenclatural stability to grant priority to *Procynosuchus* Broom, 1937 over *Parathrinaxodon* Parrington, 1936.

7. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to give precedence to the following names:
 - (a) *Procynosuchus* Broom, 1937 over the names *Cyrbasiodon* Broom, 1931 and *Parathrinaxodon* Parrington, 1936 whenever it and either of the other two are considered to be synonyms;
 - (b) *delaharpeae* Broom, 1937, as published in the binomen *Procynosuchus delaharpeae* (specific name of the type species of *Procynosuchus* Broom, 1937), over the names *boycei* Broom, 1931, as published in the binomen *Cyrbasiodon boycei* (specific name of the type species of *Cyrbasiodon* Broom, 1931), and *proops* Parrington, 1936, as published in the binomen *Parathrinaxodon proops* (specific name of the type species of *Parathrinaxodon* Parrington, 1936), whenever it and either of the other two are considered to be synonyms;
- (2) to place on the Official List of Generic Names in Zoology the following names:
 - (a) *Procynosuchus* Broom, 1937 (gender: masculine), type species by monotypy *Procynosuchus delaharpeae* Broom, 1937, with the endorsement that it is to be given precedence over the names *Cyrbasiodon* Broom, 1931 and *Parathrinaxodon* Parrington, 1936 whenever they are considered to be synonyms;
 - (b) *Cyrbasiodon* Broom, 1931 (gender: masculine), type species by monotypy *Cyrbasiodon boycei* Broom, 1931, with the endorsement that it is not to be

given priority over the name *Procynosuchus* Broom, 1937 whenever the two are considered to be synonyms;

- (c) *Parathrinaxodon* Parrington, 1936 (gender: masculine), type species by monotypy *Parathrinaxodon proops* Parrington, 1936, with the endorsement that it is not to be given priority over the name *Procynosuchus* Broom, 1937 whenever the two are considered to be synonyms;
- (3) to place on the Official List of Specific Names in Zoology the following names:
 - (a) *delaharpeae* Broom, 1937, as published in the binomen *Procynosuchus delaharpeae* (specific name of the type species of *Procynosuchus* Broom, 1937), with the endorsement that it is to be given precedence over the names *Cyrbasiodon boycei* Broom, 1931 and *Parathrinaxodon proops* Parrington, 1936 whenever it and either of the other two are considered to be synonyms;
 - (b) *boycei* Broom, 1931, as published in the binomen *Cyrbasiodon boycei* (specific name of the type species of *Cyrbasiodon* Broom, 1931), with the endorsement that it is not to be given priority over the name *Procynosuchus delaharpeae* Broom, 1937 whenever the two are considered to be synonyms;
 - (c) *proops* Parrington, 1936, as published in the binomen *Parathrinaxodon proops* (specific name of the type species of *Parathrinaxodon* Parrington, 1936), with the endorsement that it is not to be given priority over the name *Procynosuchus delaharpeae* Broom, 1937 whenever the two are considered to be synonyms.

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Comments on this case are invited for publication (subject to editing) in the *Bulletin*; they should be sent to the Executive Secretary, I.C.Z.N., c/o Natural History Museum, Cromwell Road, London SW7 5BD, U.K. (e-mail: iczn@nhm.ac.uk).

Comment on the proposed conservation of the usage of the generic name of *Drosophila* Fallén, 1823 (Insecta, Diptera)

(Case 3407; see BZN 64: 238–242, 65: 55–56, 137–150, 214–215, 304–307)

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The nomenclature of the genera related to *Drosophila*, as used in recent taxonomic papers, is based on the paper of Sturtevant (1942). However, Throckmorton (e.g. 1962) and subsequent authors recognised that the taxonomic relationships within the group are partly different and showed them on cladograms, without suggesting nomenclatural changes. Now it is quite apparent that the conception of Throckmorton is, in principle, correct and its modifications are presently discussed. After forty seven years, time is now more than mature to reflect the changes in the nomenclature. If the Commission maintains *Drosophila funebris* (Fabricius, 1787) as type species of the genus *Drosophila* Fallén, there are two possibilities how to reflect the situation: 1. *Sophophora* Sturtevant, 1939 would be elevated to the rank of genus and consequently many geneticists, physiologists, etc. would have to adopt the name *Sophophora melanogaster* for their favourite research subject. 2. Alternatively, more than ten currently independent genera would be connected with the genus *Drosophila* (see comment by P. Štys, BZN 65(2): 144). In this case, the currently independent genera *Scaptomyza*, with about fifteen subgenera, and probably *Hypselothyrea*, *Phorticella* and *Zaprionus*, with two subgenera each, would become part of the genus *Drosophila*. It is evident that the names of these genera, and/or their subgenera, could not then be used as genus-group names.

On the other hand, accepting the proposition to use the name *Drosophila* s. s. for the genus based on *D. melanogaster* Meigen, 1830 would also bring various constraints, as discussed in BZN 65(1) and 65(2). Apparently there is no perfect solution and I do not intend to anticipate the decision of the Commission. However, if *D. melanogaster* is to be accepted as type species of the genus *Drosophila*, the name of the present subgenus *Drosophila* (based on *Musca funebris* Fabricius, 1787) has to be changed at the same time. This nomenclatural act needs a comment.

Apparently no suitable genus-group name, other than *Drosophila*, has ever been used for the present type species *D. funebris*. The only available name, *Oinopota* Kirby in Kirby and Spence, 1815, is based on *Musca cellaris* Linnaeus, 1758. Type material of *M. cellaris* does not exist. Although Meigen (1830) considered *M. cellaris* synonymous with *M. funebris* Fabricius, 1787, most subsequent authors have not accepted this synonymy and *M. cellaris* is considered a nomen nudum (e.g. Brake & Bächli, 2008). Moreover *M. cellaris* was not originally included in *Drosophila* and, in my opinion, cannot become the type species of the taxon currently known as *Drosophila* s.str.

There are several genus-group names used for various subordinate taxa of the present genus *Drosophila*. The names *Idiomyia* Grimshaw, 1901 and *Hypenomyia* Grimshaw, 1901 should not be taken into consideration; they have been used for endemic Hawaiian species now considered by many authors, including Brake &

Bächli (2008), to belong to the genus *Idiomyia* Grimshaw, although this arrangement is not unequivocally accepted. Consequently the two names used by Duda, 1923 are the candidates. In the application (BZN 64(4): 239), the name *Chaetodrosophilella* Duda, 1923 (p. 40) is suggested for the present subgenus *Drosophila* (anticipated there to be raised to the genus level). However, *Chaetodrosophilella* has been used as a valid name only once before, notably for the group based on, and including only, *Drosophila quadrilineata* de Meijere, 1911 from southeast Asia and Micronesia, and has already been replaced by *Chaetodrosophila* Duda, 1924a in his subsequent publication. Therefore I do not consider it very suitable for a subgenus of several hundred species. The other genus-group name used by Duda, 1923 (p. 47) is *Spinulophila*. Duda soon transferred to *Spinulophila* the common cosmopolitan species *immigrans* Sturtevant, 1921 (misidentified as '*tripunctata* Loew, 1862: Becker, 1908' by Duda, 1924a, but recognized as '*D. immigrans* Sturtevant ?' by Duda, 1924b), which is often used as an object of genetic, physiological and ecological research, and the term '*immigrans* group' has been commonly used for more than sixty years (Sturtevant, 1942) for a group currently comprising almost 100 species. The '*immigrans* group' is close to the '*funnebris* group' and all taxonomic treatments show that they are congeneric. Furthermore *Spinulophila* has been, from the very beginning, used for a group which is not monotypic.

Other genus-group names used for subordinate taxa of the present *Drosophila* (*Acrodrosophila* Duda, 1924a, *Spinodrosophila* Duda, 1924a, *Sordophila* Wheeler, 1949, disregarding unjustified emendations) are younger. They are also apparently disqualified because they represent groups of one to several species limited to one of the zoogeographical regions (considering the Holarctic region as a single unit). The same applies to the names presently used for the other extant subgenera of *Drosophila*, which moreover represent taxa unrelated to *Drosophila funnebris*.

In summary: if the name *Drosophila* were to be used for the genus based on *D. melanogaster* Meigen, 1830, I suggest giving the present subgenus *Drosophila* the name *Spinulophila* Duda, 1923 (although this name does not have page priority over *Chaetodrosophilella* Duda, 1923), mainly because *Spinulophila* originally represents a well known group of numerous species, including one cosmopolitan species and several others important for genetic research, the group being clearly congeneric with *D. funnebris* (Fabricius, 1787), type species of *Drosophila*.

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Comment on the proposed conservation of AULACOSCELINAE Chapuis, 1874 (Insecta, Coleoptera, ORSODACNIDAE or CHRYSOMELIDAE)
(Case 3398; see BZN 65: 97–105).

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Despite having studied Greek and Latin in High School and been good at those, I prefer the traditional approach and I am 100% for MAGASCELINAE, LAMPROSOMINAE and AULACOSCELINAE, so I support this proposition.

Comment on the proposed precedence of the generic name *Ataenius* Harold, 1867 over *Aphodinus* Motschulsky, 1862 (Insecta, Coleoptera)
(Case 3377; see BZN 64: 39–42; 123; 65: 307–309)

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I strongly support the conservation of the name *Ataenius* Harold, 1867 because changing a name for one of the largest genera of SCARABAEIDAE, that is amongst the most frequently used scarab names in the taxonomic, ecological and applied literature and has even been adopted as a common name for an important pest species, would cause widespread confusion. This confusion would be even more severe since its senior synonym, *Aphodinus* Motschulsky, 1862 shows only a one letter difference to an even more frequently used scarab name, *Aphodius* Illiger (with counts for 186 references between 2002 and 2008 in Zoological Record, searched on 28 December 2008).

Ataenius has continuously and frequently been used since its description. Howden & Smetana's (BZN 64: 40) statement that this name has been used in at least nine papers since the synonymy between *Aphodinus* and *Ataenius* was established by Dellacasa et al. (2001) is an underestimate. A list of 126 papers using *Ataenius* as valid between 2002 and 2008 is held by the Secretariat. Howden & Smetana (BZN 64: 40) also state that *Ataenius* has a history of use in over a hundred references. Since this name has been used at least 126 times during the last eight years alone, this estimate is likely to be wrong by an order of magnitude. *Aphodinus* Motschulsky has obviously not gained recognition in the scientific community. It has apparently been noted but deliberately rejected by leading taxonomists. Stebnicka (2007) lists *Aphodinus* as a senior synonym of *Ataenius* but considers it incorrectly as a nomen oblitum. Smith & Skelley (2007, p. 33) also list *Aphodinus* as an older synonym, and likewise use *Ataenius* as valid. Ratcliffe & Paulsen (2008) cite both those publications but do not even mention *Aphodinus* and use *Ataenius* as valid. The only authors to consider *Aphodinus* Motschulsky as valid with '*Ataenius* Harold, 1867: 100 (*partim*)' as a junior synonym are Dellacasa et al. (2001, p. 36) but they are unsure whether the type species of *Aphodinus* is congeneric with the type species of *Ataenius*, and one of them expressed support for Case 3377 in a comment in BZN 65: 307–308.

Fifteen references documenting use in the taxonomical literature were given in the Application (BZN 64: 40) and the leading authority on this taxon, Z. Stebnicka, indicates about 150 taxonomical references in her comment (BZN 64: 123). *Ataenius* is frequently mentioned in the ecological literature also (e.g. Deloya et al., 2007; Koller et al., 2007; Steinbauer & Weir, 2007; Utz et al., 2007; Horgan, 2008; Pawson et al., 2008). *Ataenius spretulus* (Haldeman) is a common turfgrass pest in Ontario and the United States, reported from 41 states and causing damage on golf courses in at least 23 states (Tashiro, 1987; Jo & Smitley, 2006) resulting in an extensive body of applied literature, e.g. Weaver & Hacker (1978), Vittum (1995, p. 35), Kido et al. (1996, p. 12), Smitley & Davis (1999, p. 53), Labonte (2002), Dreistadt et al. (2003, p. 2), Köppen-höfer et al. (2004, p. 88), Williamson et al. (2005, p. 803), Baghzouz et al. (2006, p. 4139), Ontario Ministry of Agriculture Food and Rural Affairs (2008, p. 4). *Ataenius* has even been adopted as a common name in the form of 'black turfgrass ataenius'/'black turfgrass *Ataenius*' or 'BTA' in the pest control literature (e.g. Grewal, 1999, p. 288; Willmott, 2000; Rogers & Potter, 2002, p. 12; Fresenburg et al., 2003: 7; Hodgson, 2007, pp. 1–2 and most other references mentioned in this paragraph) and has been approved as common name by the Entomological Society of America (Werner, 1982, p. 7; Bosik, 1997, p. 13). *Ataenius* is not only a commonly used name in pest control, but some *Ataenius* species are threatened and have entered the conservation literature. *Ataenius superficialis*, the Big Pine Key Dung Beetle, and *A. woodruffi*, Woodruff's Dung Beetle, were listed by the IUCN as vulnerable and endangered, respectively (Groombridge, 1993, p. 171). These and other *Ataenius* species are used in the conservation and planning literature (Woodruff & Deyrup, 1994a–f; Deyrup, 1994; Drewry, 1994; MacAllister & Harper, 1998, p. 26; Mazzotti et al., 2002; Scott, 2003, p. 72).

The change of such a widespread name because of a subsequent type species designation in another genus causing *Ataenius* to be a junior subjective synonym should be avoided, particularly since one of the authors of this designation does not appreciate its consequences. Had Dellacasa et al. (2001) chosen another one of the originally included species, *Aphodius compacticollis* Motschulsky, as type species, *Ataenius* would not be threatened. Asking the Commission to use its plenary power to change the type species of *Aphodius* to *A. compacticollis* would mitigate, but not solve the problem. *Ataenius* would no longer be threatened but the well-established though less frequently used genus-group name, *Aganocrossus* Reitter, 1895, would be instead. *A. compacticollis* was claimed to be a senior synonym of *A. urostigma* Harold, 1862 (Kozhantshikov, 1916, p. 192) which belongs to this subgenus or genus. However, this synonymy needs revision (Bordat & Dellacasa, 1996, p. 148). With the taxonomy of *A. compacticollis* being unresolved, changing the type species of *Aphodius* is no feasible alternative to Howden & Smetana's original proposal (BZN 64: 40).

Branco & Dellacasa (BZN 65: 307–308) supported maintaining *Ataenius* as a valid name but claimed that it was threatened by another senior synonym, *Auperia* Jacquelin-Duval, 1857, because the type species of this genus, *Scarabaeus stercorator*, designated by Dellacasa himself in 1988, is currently included in *Ataenius*. However, Dellacasa's type species designation is invalid. *Auperia* was introduced by Jacquelin-Duval (1857, p. 50) as replacement name for '*Euparia*, Erichs., Arch. f. Naturg., 1847, I, 110' on the grounds that the name *Euparia* had been used before by Le Peletier de Saint-Fargeau & Serville and the name *Euparius* by Schönherr. However, *Euparius* is not a homonym of *Euparia* (Article 56.2), and Erichson (1847, p. 110) did not

describe a new genus *Euparia*, but simply used ‘*Euparia* Lepell. et Serv. Encycl. meth. X. 1825’ in which he described two new species. There is no genus ‘*Euparia* Erichson’, only *Euparia* Le Peletier de Saint-Fargeau & Serville. Since the author is not part of the scientific name (Article 51.1) but only a bibliographic reference, *Euparia* as used by Erichson as subsequent user (Article 51.2.1) is actually *Euparia* Le Peletier de Saint-Fargeau & Serville, thus *Auperia* Jacquelin-Duval being a replacement name for the latter. According to Article 67.8 *Auperia* Jacquelin-Duval, as a replacement name, has the same type species as the genus it is intended to replace. The type species of *Euparia* Le Peletier de Saint-Fargeau & Serville is *Euparia castanea* Le Peletier de Saint-Fargeau & Serville, 1828, by monotypy (Le Peletier de Saint-Fargeau & Serville, 1828, p. 357). The type species of *Auperia* Jacquelin-Duval is the same. It is irrelevant that *Auperia* was an unnecessary replacement name since Article 67.8 applies to all nomina nova. Therefore, *Auperia* is a junior objective synonym of *Euparia* and does not threaten *Ataenius*.

Stebnicka (2002, p. 742) erroneously considered *Auperia* Jacquelin-Duval a nomen nudum although it was introduced as a replacement name, hence fulfilling the requirements of Article 12.2.3 to be available by indication. She uses the name *Auperia* Chevrolat, 1864 as valid and she designates *A. denominata* Chevrolat, 1864 as its type species (according to Article 70.3). However, Chevrolat (1864, p. 413) did not describe a new genus but referred to Jacquelin-Duval’s *Auperia* (‘*Auperia stercorator* Jac. Duval, loc. cit., p. 117’). There is no ‘*Auperia* Chevrolat’ and Stebnicka’s type species designation is invalid since it relates to *Auperia* Jacquelin-Duval the type species of which is determined by the type species of the name it was supposed to replace (Article 67.8). No action of the Commission is required relating to the genus-group name *Auperia*. Consequently, Branco & Dellacasa’s other requests (BZN 65: 308) are also unnecessary.

I suggest that the Commission votes on Howden & Smetana’s original requests (BZN 64: 40) with the only correction that, as indicated by Branco & Dellacasa (BZN 65: 307), the type species of *Ataenius* was first designated by Chapin (1940) and not Cartwright (1974).

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Comments on the proposed conservation of *Buettneria* Case, 1922 (Amphibia)
(Case 3420; see BZN: **64**: 252–254, **65**: 60–62, 217–219, 310–314)

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I oppose the petition of Lucas et al. for the Commission to use their plenary power to conserve *Buettneria* Case, 1922 in preference to either of the senior homonyms. Both the initial proposal to conserve *Buettneria* Case, 1922 and the comments supporting this proposal ignore the full content of Article 23.9.1 of the Code. This

includes Article 23.9.1.1 stating that a precondition for consideration is that the senior homonym has not been used as a valid name after 1899. *Buettneria* Karsch, 1888 (1889) was used by Ragge (1962) and *Buettneria* Simroth, 1888 was used by Van Goethem (1975 et seq.) and Schileyko (2002), as noted by Hausdorf (BZN 65: 313). Thus neither of the senior homonyms of *Buettneria* is a nomen oblitum, and to conserve *Buettneria* Case, 1922 in preference to either would be to overturn Article 23.9.1.1. This in turn would be an effective statement that the Law of Priority no longer applies to infrequently studied taxa, of which there are many hundreds of thousands. I suggest that this wider implication should be given consideration by the Commission when they consider this issue. Article 23.9.1.1 exists in this explicit form to protect the priority of infrequently studied taxa. The proposal for conservation of *Buettneria* Case, 1922 uses the disingenuous but meaningless term ‘virtual nomen oblitum’ to describe *Buettneria* Karsch, 1888 (1889) to evade the basic issue that it is not a nomen oblitum and is protected by Article 23.9.1.1. In my view, Article 23.9.1.1 precludes all consideration of conservation of *Buettneria* Case, 1922.

Discrimination between the competing claims of *Buettneria* Karsch, 1888 (1889) and *Buettneria* Simroth, 1888 is a more legitimate topic for the Commission to consider and falls within the realms of opinion. My view is that i) Simroth himself replaced the name *Buettneria* with *Buettnerella* and that if, as original author, he felt it necessary to do this, this should have been respected by later workers; ii) when Van Goethem (1975) revived *Buettneria* Simroth, 1888, he failed to treat it as a nomen oblitum as it was under Article 23b.i in the prevalent (1964) edition of the Code of Nomenclature. In that edition of the Code, which would have still been operative in 1975, the fifty year rule defined nomina oblita. The history of *Buettneria* Karsch, 1888 (1889) may be brief but it is immaculate.

In conclusion, I support Mueller’s (2007) published proposal that the junior homonym *Buettneria* Case, 1922 be replaced by its junior synonym *Koskinonodon* Branson & Mehl, 1929. My preference would be for the seniority of *Buettneria* Karsch to be recognised, but under Article 23.9.1.1 both of the senior homonyms have a stronger case than Case’s juniormost homonym.

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As a zoologist working on invertebrates, I feel belittled by the nature of the arguments put forth in favour of conserving the name *Buettneria* Case, 1922, against *Buettneria* Simroth, 1888. The technical arguments have by now been, in my opinion, adequately addressed in the comments published on this case. At face value, the number of usages is indeed overwhelmingly in favour of *Buettneria* Case, 1922, but I would argue that this comparison is unfair. We have, on one hand, a fossil vertebrate from the land of plenty, North America, and on the other hand, a land snail from the heart of Africa. It does not take a diviner or advanced bibliometrics to predict that the former will get considerably more attention than the second. When measured by museum exhibits, graduate courses, and scientific expertise available, the land snails are bound to lose against vertebrates. The name *Buettneria* Simroth, 1888, accounts for a minuscule number of usages not because it is a ‘virtual nomen

oblitum', but simply because it is the fate of most invertebrate taxa to attract very limited attention. The urocyclids are a very diverse family of semi-slug land snails in Africa south of Sahara, with hundreds of described species and certainly hundreds more to be described. Yet, the monographs of the family by van Goethem (1977) and van Mol (1970), now over 30 years old, have barely been outdated by subsequent additional publications. In the case of *Buettneria leuckarti* Simroth, 1888, the type species described from Angola, no field work has been carried out in that country for decades because of the civil war and insecurity.

There is no article in the Code that says that the name of a bird or a primate should have priority over the name of a mite or a snail. The name *Buettneria* Simroth, 1888, has not been rediscovered as a result of book archaeology. Instead, it was captured by nomenclators, it appeared in relevant treatises and textbooks, and it was used by the zoologists when they had to deal with the taxon the name applies to. Vertebrate palaeontology survived the name *Brontosaurus* giving way to *Apatosaurus*, so I see no reason why *Buettneria* Simroth, 1888 should now be displaced by *Buettneria* Case, 1922.

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Comment on the proposed precedence of *Anolis chrysolepis* Duméril & Bibron, 1837 (Reptilia: Squamata) over *Draconura nitens* Wagler, 1830
(Case 3446; see BZN **65**: 205–213)

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As a taxonomist actively studying the systematics and biogeography of South American reptiles, I strongly support the proposal by Myers (BZN **65**: 205–213) that the Commission use its plenary power to give the name *chrysolepis* Duméril & Bibron, 1837 (published as *Anolis chrysolepis*) precedence over the name *nitens* Wagler, 1830 (published as *Draconura nitens*) when the two names are considered synonyms. The name *nitens* Wagler (1) cannot be unambiguously applied to any known taxon; (2) has been applied to diverse populations spread across a large region of South America; (3) is not tied to a specific taxon because its type specimen has been lost; and (4) perhaps applies to a forgotten lizard species with a distinctive colour pattern unknown in any of the forms of *Anolis chrysolepis*. *D. nitens* Wagler is a nomen dubium as defined by the Code. In contrast, the name *chrysolepis* (1) unambiguously applies to a particular taxon and has an extant lectotype; (2) has been in use for nearly the same amount of time as the name *nitens*; and (3) was used in a landmark taxonomic revision that applied the concept of refugia to South America; the name was subsequently used in nearly half a century of literature on South American biogeography as well as other systematic studies. For these reasons, *Anolis chrysolepis* should have precedence over *Draconura nitens* when the two names are considered synonyms.

Comments on the proposed precedence of *Chelodina rugosa* Ogilby, 1890 (currently *Macrochelodina rugosa*; Reptilia, Testudines) over *Chelodina oblonga* Gray, 1841 (Case 3351; see BZN 63: 187–193, 64: 68, 127–128, 65: 62)

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Having studied the original application as well as succeeding comments, including J.M. Savage's views, I strongly support the original proposals submitted by S.A. Thomson (BZN 63: 187–193). Historical confusion in the application of names for turtles in various regions of Australia and Papua was, and still is, primarily caused by unresolved problems in taxonomy. This is illustrated by the fact 'that half of the species of turtles from Australia have been described since 1980, and all but one of these since 1994' (Thomson, 2007, BZN 64 128). The taxonomy of populations in Northern Australia and Papua still needs clarification. Under such conditions and as already stated by U. Fritz (BZN 65: 62), the artificial attempt to conserve previous usage by a neotype selection would simply conserve a single case within a whole series of historical misapplications (and affect the status of the name *Macrochelodina* Wells & Wellington, 1985). The original application combines a minimum of nomenclatural change with a maximum of future taxonomic freedom.

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I am writing in support of the proposed precedence of *Chelodina rugosa* Ogilby, 1890 over *Chelodina oblonga* Gray, 1841. Further, I support usage of the available name *Chelodina colliei* Gray, 1856 for the species known as *Chelodina oblonga* Gray, 1841 that has been misapplied for the past 40 years.

Savage (BZN 64: 68) suggested in his application to the Commission to set aside all previous designations of a type specimen for *Chelodina oblonga* Gray, 1841 and to designate as its neotype BMNH 1947.3.5.91, the lectotype of *Chelodina colliei* Gray, 1856.

Recent work in our lab, contained in the recently submitted Ph.D. thesis of Erika Alacs and soon to be published, shows two quite divergent clades within what is currently regarded as *Chelodina rugosa* Ogilby 1890. One distinct clade has a geographic range covering much of the northern territory, and almost certainly includes the type locality of *Chelodina oblonga* Gray, 1841. The other distinct clade has a geographic range covering southern New Guinea and Cape York, which includes the type locality of *Chelodina rugosa* Ogilby, 1890. I do not regard the populations containing these divergent mitochondrial haplotypes as biological species, but it certainly opens the real possibility that they may be demonstrated to be so in future studies involving a broader range of genetic and morphological markers.

This presents a clear problem if the solution suggested by Savage (BZN 64: 68) is adopted. Two named taxa with type specimens drawn from the same biological species would be undesirable in our view, and the possibility should not be admitted.

The proposition by Thomson is sensible in that it eliminates the problem as outlined above, and gives appropriate recognition to the person who originally described the south-western Australian species as *Chelodina colliei* Gray, 1856 (used for 136 years). Thomson's Case 3351 and Comment in BZN 64: 127–128 are reasonable and provide the best solution to this nomenclatural problem.

Comments on the proposed conservation of usage of *Testudo gigantea* Schweigger, 1812 (currently *Geochelone (Aldabrachelys) gigantea*; Reptilia, Testudines)
(Case 3463; see BZN 66: 34)

(1) George R. Zug

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Smithsonian Institution, Washington DC, 20013–7012 U.S.A. (email: zug@si.edu)*

I support the application of Frazier to conserve *Geochelone gigantea* Schweigger, 1812 by the acceptance of a neotype designation. Frazier (2006a, 2006b) detailed the confusion and nomenclatural instability that has resulted from Bour's (1982) proposal of *Dipsochelys*. While some turtle systematists have followed Bour's proposal, others have not, and the general biological community, especially ecologists and herpetoculturists, have continued to use *Geochelone gigantea* or *Aldabrachelys gigantea*. Given the uncertainty regarding the specific identity of the type, which was poorly illustrated, Bour upset nomenclatural stability of the Aldabran giant tortoise by his failure to follow the principle of the earlier Code (1961 – Article 23b(iii)) to request an evaluation by the Commission on the advisability of changing the widely used (i.e. outside the turtle community) name *Geochelone gigantea* that had instant recognition by the public and biologists for nearly three-quarters of a century. Frazier's (2006a) designation of a neotype fixes the name *Geochelone gigantea* Schweigger to the giant tortoises of Aldabra. The discovery of Schweigger's purported type specimen (Bour, 2006) immediately followed the designation of the neotype, and the publication of this discovery continues to foster nomenclatural confusion. The chain of evidence for the purported holotype has too many uncertainties, and these will continue to cause arguments on the assignment of the nomen *gigantea* whereas the acceptance of the neotype designation resolves the problem and promotes nomenclatural stability.

(2) Katy Beaver

*Plant Conservation Action group & L'Ilot, Glacis (or PO Box 392), Victoria,
Mahe, Seychelles (e-mail: kbeaver@seychelles.net)*

I would like to support the petition to conserve the name of the Aldabra tortoise as *Testudo gigantea*.

As secretary of the Science Committee of the Seychelles Islands Foundation (which manages Aldabra Atoll) for some years, it was often frustrating to find so many names given to this animal icon of the atoll. The use of the specific name *gigantea* I support unreservedly.

In education and awareness work, which I am much involved with, in particular for local people and for tourists, it has been a constant headache to know how or

what to call the Giant Tortoise, which is important both in local culture and history, and also as a famous and prominent animal found in Seychelles. Sometimes it is not necessary to use a Latin name, but when this information is given for all other species in the text, it seems odd not to put one for the Giant Tortoise. Most people cannot fathom why there should be so much fuss made over a Latin name, and the fact that there have been so many different names is quite an embarrassment! For the sake of sanity as well as science I welcome this petition and fully support it.

(3) Karen A. Bjorndal

Department of Zoology, PO Box 118525, University of Florida, Gainesville, FL 32611, U.S.A. (e-mail: kab@zoology.ufl.edu)

I support the petition to conserve the specific name *Testudo gigantea* Schweigger, 1812, for the Aldabra tortoise. I agree with Frazier that *T. gigantea* is the established name. As explained in Case 3463, it has been in continuous use for more than 100 years and has been widely used in the scientific literature. It is important to settle this nomenclatural issue because there has been considerable confusion since 1982. The neotype for *T. gigantea* (USNM 269962) that was designated in 2006, should be maintained.

(4) Charles R. Crumly

University of California Press, 2120 Berkeley Way, Berkeley, CA 94704 USA. (e-mail: chuck.crumly@ucpress.edu)

I write in strong support of the petition by Jack Frazier to conserve the usage of *Testudo gigantea* Schweigger, 1812 by the designation of a neotype. Further, I agree that *Testudo dussumieri* Gray, 1831 should be suppressed.

The arguments of Frazier are compelling, logical, well-reasoned, and will result in the least amount of confusion – especially among scholars unfamiliar with the ancient, arcane, difficult to interpret and sometimes difficult to access literature. The most obvious advantage of adopting Frazier's application is the stabilisation of a name familiar and in wide use for over 100 years. In the last 25 years, great confusion has prevailed, partly caused by those that support a name other than *gigantea* for the Aldabra tortoise. Indeed, those that have rejected *gigantea* have, with equal misplaced certainty, suggested significantly different flawed proposals.

The need to stabilise the name for the Aldabra tortoise is becoming more and more serious due to the potential risk of extinction. The only extant population is on a remote and isolated island and is vulnerable. Hypothesised sea level changes represent a real threat to a low lying island such as Aldabra. Maintenance of the neotype with its documented collection locality on Aldabra is the most convenient and least ambiguous means by which to preserve the best name for Aldabra tortoises. The usage of *Testudo gigantea* Schweigger, 1812 for the Aldabra tortoise is obvious, appropriate and least disruptive, and should be conserved.

(5) Indraneil Das

Institute of Biodiversity and Environmental Conservation, Universiti Malaysia Sarawak, 94300 Kota Samarahan, Sarawak, Malaysia (e-mail: hamadryad2004@hotmail.com)

I am writing in support of the petition by J. Frazier to conserve the specific name *Testudo gigantea* Schweigger, 1812, in the interest of nomenclatural universality and stability. The nomen *Testudo gigantea* for the Aldabra tortoise, a species of great conservation concern, has been in use for over a century. Thus, resurrection of the nomen oblitum *Testudo dussumieri* Gray, 1831 and invalidation of neotype designation for *T. gigantea* is not deemed worthy of support, particularly since the rediscovery of the supposed holotype is not unequivocally proven.

(6) A.W. Diamond

Wildlife Ecology, University of New Brunswick P.O. Box 4400, Fredericton, NB, Canada E3B 5A3 (e-mail: diamond@unb.ca)

I write briefly in support of the case made by J. Frazier for the conservation of the specific name *Testudo gigantea* Schweigger 1812 and suppression of *Testudo dussumieri* Gray 1831.

I have read J. Frazier's case carefully in its entirety, and am impressed by the rigour with which he has navigated what can only be described as the tortuosities of tortoise nomenclature. His credentials are impeccable and his case incontrovertible.

Great confusion has been caused in this area recently and there is a clear need to set the record straight as J. Frazier proposes.

(7) Clive Hambler

Department of Zoology, University of Oxford, South Parks Road, Oxford OX1 3PS, U.K. (e-mail: clive.hambler@zoo.ox.ac.uk)

I very strongly support the petition from Frazier to stabilise the nomenclature of this species in the relatively straightforward way proposed, using USNM 269962 as the neotype of *T. gigantea*.

The species name *gigantea* is very widely used, far more than any alternatives, and has been for decades. Almost all the ecologists who work on this species, including myself, have used and still use the name *Geochelone gigantea* (e.g. Bourn et al., 1999). There has been very unwelcome confusion in some quarters about the name, with proposals to use *Dipsochelys elephantina* or *Dipsochelys dussumieri*, although most experts on the ecology of the species have ignored this. The confusion was exacerbated by the failure of some authors to appreciate phenotypic plasticity in the species.

As the petition demonstrates there have been a number of proposed name changes, which hopefully will not be adopted and all of which I have ignored (fortunately, as it proves). To minimise further confusion, I advise people to continue to use *gigantea* unless there is an overwhelming reason not to. I would be far more at ease with a proposed change of generic name (if taxonomic evidence became strong enough) than a different species name.

As an ecologist and conservationist, I want strong links to be retained between the diverse publications on this species which is IUCN Red Listed and which arouses wide public interest. I have used the name *Geochelone gigantea* for several references to this species in my own textbook on conservation (Hambler, 2004, pp. 7, 361) and

other publications (Hambler et al., 1985, 1993; Hambler, 1994; Linfield et al., 1993; Seaward et al., 1996). Both editions of a major student textbook on biogeography (Whittaker, 1998, 2007) also use this name. It is readily apparent that the species name *gigantea* is embedded in a wide range of literature from different biological disciplines which students may encounter early in their studies.

In addition to literature which is easy to find the name *gigantea* is used in a number of unpublished reports from British university student expeditions (e.g. from Oxford in 1983, 1988, 1990 and Southampton in 1982), available from the Royal Geographical Society (with the Institute of British Geographers) and in various university libraries.

In teaching at undergraduate level and above, and in conservation literature, I advise that until there is a decision from the Commission it is highly desirable to continue to use *gigantea*; this will make links between publications, and ecological progress, much easier for students and practitioners. It is not in the interests of education or biology to maintain the current volatility which has already gone on too long. To my mind, the main value of pointing students towards the other names is as an illustration of the great intricacies of some scientific and nomenclatural controversy.

Additional references

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(8) Thomas Leuteritz

Monitoring and Assessment of Biodiversity Program, National Zoological Park, Smithsonian Institution, 1100 Jefferson Drive SW, Suite 3123, Washington, DC 20560–0705, U.S.A. (e-mail: tejl@hawaii.edu)

I support the proposed conservation of usage of the specific name *Testudo gigantea* Schweigger, 1812, by maintenance of a designated neotype, and suppression of *Testudo dussumieri* Gray, 1831. I agree with Frazier (BZN **66**: 34) that *T. gigantea* is

the established name for the Aldabra tortoise (i.e. it has been in continuous use for over 100 years, and it has been recognised as the oldest name for the Aldabra tortoise for more than 50 years). Furthermore, as Frazier documented, there has been much nomenclatural chaos, and it is necessary to stabilise the nomenclature of the Aldabra tortoise.

(9) Nirmal Jivan Shah

Nature Seychelles, P.O. Box 1310, The Center for Environment and Education, Roche Caiman, Mahe, Seychelles (e-mail: nature@seychelles.net)

We wish to support the petition by Jack Frazier. We strongly recommend that *Testudo gigantea* be used to refer to the Aldabra Giant Tortoise. We note that this is the name Gazetted by the Seychelles Government in 'Wild Animals (Giant Land Tortoises) Protection (Amendment) Regulations (1999. S.I. 39 of 1999)'.

T. gigantea has been used for over 100 years in hundreds of publications thus proving it is the established name. The current confusion in nomenclature caused by only a few authors is proving to be more than a nuisance and hindering progress in science and conservation. It is therefore a high priority to have one established name for the Aldabra Giant Tortoise. We believe it would be parsimonious to standardise the name as *Testudo gigantea*.

(10) James B. Murphy

Department of Herpetology, National Zoological Park, Smithsonian Institution, Washington, DC 20560-0705, U.S.A. (e-mail: jbmurphy2@juno.com)

Please consider my comments on Case 3463: (1) *Testudo gigantea* is the established name for the Aldabra tortoise (i.e. it has been in continuous use for over 100 years, it has been recognised as the oldest name for the Aldabra tortoise for more than 50 years, it is widely used in the literature, etc.; (2) there has been nomenclatural chaos, particularly since 1982; (3) it is necessary to stabilize the nomenclature and fix the name of the Aldabra tortoise and (4) the simplest way to achieve this is by maintaining the neotype for *T. gigantea* (USNM 269962) that was designated in 2006.

(11) Ian R. Swingland

Ultra Green Group, Singapore; Conservation Research & Development, Mauritius; Sustainable Forestry Management, London; Moulin Vert, Normandy; DICE, Kent; Earthwatch Institute, Boston and Oxford; Darwin Initiative, UK Government, London; Herons Hall, Nash, Canterbury, Kent CT3 2JX, U.K. (e-mail: ian@herons-hall.co.uk)

I fully support the retention of the name *Geochelone gigantea* for two reasons: i) it must retain a common usage stability which is allowed within the ICZN rules, and ii) I am tired of constant changes being made to the name as though it was a game, no doubt for very legitimate taxonomic reasons. The arguments and precedents and parsimony analysis may have produced all sorts of errors in the past but there are

many other scientists working on the chelonians in ecology, behaviour, genetics, population studies, and other aspects of their biology who find all this indecision and constant changing of the Latin binomial very unhelpful to the science or the conservation of the species. I will always use *Geochelone gigantea* as a matter of principle and clarity however erroneous that is. We know what species we are dealing with here and there is no risk of confusion to misdirect conservation efforts; I must say the idea of fiddling about with names, when tortoises are burning and could use our taxonomic help, I find very distasteful. I worked on this species for 10 years, two of which I lived on Aldabra Atoll. I was later the Founding Chairman of the IUCN SSC Tortoise Specialist Group, and then Director of the First World Congress of Herpetology. Please accept my total support for this move to retain the original name in common usage.

(12) David Bourn

*Food and Agriculture Organization of the United Nations, 29th Floor,
Yuchengco Tower 1, RCBC Plaza, 6819 Ayala Avenue, Makati City 1200,
Metro Manila, Philippines (e-mail: david.mackenzie.bourn@gmail.com)*

As a long-standing participant in various field studies of tortoises on Aldabra Atoll, Seychelles, I am pleased to support the above petition by Jack Frazier of the Smithsonian Institution. In my considered opinion, the continuing nomenclatural uncertainty has prevailed for far too long, and needs to be resolved as soon as possible to avoid perpetuating further ambiguity and confusion.

(13) Oguz Turkozan

*Adnan Menderes Universitesi, Fen Edebiyat Fakultesi, Biyoloji Bolumu, 09010
Aydin, Turkiye (e-mail: oguzturkozan@yahoo.com)*

I support the proposal to conserve the name of the Aldabra tortoise, *Testudo gigantea* Schweigger, 1812, for the following reasons: while *T. gigantea* is the established name for the Aldabra tortoise (i.e. it has been in continuous use for over 100 years and has been recognised as the oldest name for the Aldabra tortoise for more than 50 years) there has been considerable nomenclatural instability since 1982. I consider necessary to stabilise the nomenclature by maintaining the neotype for *T. gigantea* (USNM 269962) that was designated in 2006.

(14) Jay M. Savage

*Department of Biology, San Diego State University, San Diego, CA 92182-4614
U.S.A. (e-mail: savy1@cox.net)*

I wish to fully support the application by Jack Frazier relating to the name *Testudo gigantea*. Previous authors have manipulated the evidence as it relates to this name to allow them to propose new ones. Many years of usage in this case should trump speculation as to the origin of the type specimen which in any case cannot definitely be identified.

(15) Gisella Caccone

*YIBS-Molecular Systematics and Conservation Genetics Lab, ESC 140,
Yale University, 21 Sachem Street, New Haven, CT 06520–8106, U.S.A.
(e-mail: adalgisa.caccone@yale.edu)*

As a concerned scientist working for the past 20 years on evolutionary genetics and conservation of Giant Galapagos tortoises, both in Aldabra and Galapagos, I have been made aware that there is a case submitted to the Commission to fix the specific name of the Aldabra tortoises once and for all to *gigantea*.

I completely and enthusiastically support this case. It would be a great service to the scientific and management community if the name of the Aldabra tortoise were fixed and the obvious choice is the established / customary / accustomed name of *gigantea*.

In the past years there have been a series of names proposed and used which have confused the issue and misinterpreted and misused taxonomic rules. For those of us working on conservation issues this has rendered our efforts much less effective; we cannot protect what we cannot name and identify precisely.

I really hope that the Commission can decide in favour of case 3463.

(16) James F. Parham

*The Field Museum of Natural History (Biodiversity Synthesis Center),
1400 S. Lake Shore Dr., Chicago, IL 60605, U.S.A.; and Department of
Herpetology, California Academy of Sciences, 55 Music Concourse Drive, Golden
Gate Park, San Francisco, CA 94118, U.S.A. (e-mail: jparham@fieldmuseum.org)*

I support the petition described in Case 3463 to conserve the name *Testudo gigantea* Schweigger, 1812 for the Aldabra tortoise by retaining USNM 269962 as the neotype of this taxon and suppressing *Testudo dussumieri* Gray, 1831. There has been substantial nomenclatural chaos and confusion about what is the correct name for Aldabra tortoises. For over 100 years, the name *Testudo gigantea* was widely used for Aldabran tortoises. *Testudo gigantea* was designated as the type species of *Aldabrachelys* by Loveridge and Williams (1957) and the names *Geochelone gigantea* and *Aldabrachelys gigantea* remain the most widely used names for Aldabran tortoises today. This is despite the fact that Bour (1982) created confusion by claiming that the type of *Testudo gigantea* was certainly a Mascarene tortoise (genus *Cylindraspis*) and then later (Bour, 2006) claiming that it refers to a South American tortoise (genus *Chelonoidis*). The rediscovery of the holotype of *T. gigantea* is not unequivocally proven and will continue to provoke debate about the name and nomenclatural instability. Therefore the simplest solution is to maintain USNM 269962 as the neotype of *T. gigantea* and suppress *T. dussumieri* Gray, 1831, as recommended by Frazier (2006) and detailed in Case 3463.

(17) Anders G.J. Rhodin

*Chelonian Research Foundation, 168 Goodrich St., Lunenburg, MA 01462 USA
Chair, IUCN/SSC Tortoise and Freshwater Turtle Specialist Group
(e-mail: rhodincrf@aol.com)*

I support the petition by Frazier to conserve the names *Aldabrachelys* and *Testudo gigantea* for the Aldabra giant tortoise. Though strict adherence to the Code might require the name changes proposed by Bour and Gerlach, the question of the identity of the recently rediscovered purported holotype of *gigantea* makes these changes not only undesirable but also uncertain. Stabilising the nomenclature through acceptance of the unequivocally identified neotype of *gigantea* would resolve the confusion and uncertainty regarding this taxon. The name *gigantea* has been associated with Aldabra tortoises for most of the last 100 years and that stability needs to be conserved. Additionally, from a conservation perspective, destabilising the nomenclature of this threatened species (IUCN Red List status Vulnerable) increases its risk through obfuscation of its name, creating potential loophole exceptions in laws and regulations concerning its protection.

Comments on the proposed conservation of usage of *Archaeopteryx lithographica* von Meyer, 1861 (Aves) by designation of a neotype
(Case 3390; see BZN 64: 182–184, 261–262; BZN 65: 314–317)

(1) Zhonghe Zhou

Institute of Vertebrate Paleontology and Paleoanthropology, Chinese Academy of Sciences, Beijing 100044, China (e-mail: zhouzhonghe@ivpp.ac.cn)

I write to support Bock & Bühler's proposal to set aside the single feather holotype of *Archaeopteryx lithographica* in the Berlin and München Museums and to conserve the usage of *Archaeopteryx lithographica* von Meyer, 1861 (Aves) by designation of a neotype, BMNH 37001 in the Natural History Museum, London. The major reasons are as follows:

Firstly, hundreds of feathered dinosaurs have been found dating from the late Jurassic to Early Cretaceous of northeastern China in the last 15 years (Zhang et al., 2008), which indicates that presence of feathers can no longer be treated as a diagnostic character for birds, and it is possible that the holotype of *Archaeopteryx lithographica* von Meyer, 1861, represented by a single isolated feather, could be a different taxon from that now represented by several skeletal specimens referred to this species. I believe that stability is best served by conserving the usage of *Archaeopteryx lithographica* von Meyer, 1861 that has been widely known for about one and a half centuries; however, it also seems important and necessary to set aside the holotype feather and designate a neotype for this avian species.

Secondly, BMNH 37001 represents the first reported skeletal specimen of *Archaeopteryx lithographica* (Owen, 1863). This has been much more extensively studied and is possibly more commonly associated with the name than the holotype feather, so it should have the priority to be selected as the neotype specimen despite the fact that several other well preserved skeletons referable to the same species are also known (Mayr et al. 2007).

Finally, I believe that if this proposal is accepted, it will clarify a long standing historical issue in avian taxonomy, and the isolated feather that is currently the holotype of *Archaeopteryx lithographica* will be regarded as a referred specimen of this species.

Additional references

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- Zhang, F., Zhou, Z., Xu, X., Wang, X. & Sullivan, C. 2008. A bizarre Jurassic maniraptoran from China with elongate ribbon-like feathers. *Nature*, **455**: 1105–1108.

(2) Gareth J. Dyke

School of Biology and Environmental Science, University College Dublin, Dublin, Ireland (e-mail: gareth.dyke@ucd.ie)

Gary W. Kaiser

Royal British Columbia Museum, Victoria, Canada (e-mail: gansus@shaw.ca)

We support Bock & Bühler's proposal to set aside the holotype specimen of *Archaeopteryx lithographica* von Meyer, 1861 and to conserve this name by designating specimen BMNH 37001 in the Natural History Museum (the 'London specimen') as the neotype. We echo the comments in this regard made by Barrett & Milner (BZN **64**: 261–262) and add our opinion that, while a name is a useful tool, where usage is extensive, common sense is more important than hermetic bureaucracy. Vertebrate palaeontologists have for years effectively treated 'the London specimen' as the type of *Archaeopteryx lithographica* von Meyer, 1861; in any case it will likely never be possible to confidently determine whether the isolated feather (the current 'holotype' of this taxon) pertains to the same species. The Commission should make the sensible decision in this case.

OPINION 2216 (Case 3404)***Atlanta lesueurii* J.E. Gray, 1850 (Mollusca, Gastropoda, PTEROTRACHEOIDEA, ATLANTIDAE): specific name conserved**

Abstract. The specific name of the heteropod *Atlanta lesueurii* J.E. Gray, 1850 (ATLANTIDAE), originally published as a primary homonym of the pteropod *Atlanta lesueurii* d'Orbigny, 1836 (currently *Limacina lesueurii*), is hereby conserved by ruling that the name is not invalid by reason of being a junior primary homonym.

Keywords. Nomenclature; taxonomy; Gastropoda; Heteropoda; Pteropoda; PTEROTRACHEOIDEA; ATLANTIDAE; LIMACINOIDEA; LIMACINIDAE; *Atlanta*; *Limacina*; *Atlanta lesueurii*; *Limacina lesueurii*; gastropods.

Ruling

- (1) Under the plenary power it is hereby ruled that the specific name *Atlanta lesueurii* J.E. Gray, 1850 is not invalid by reason of being a junior primary homonym of *Atlanta lesueurii* d'Orbigny, 1836.
- (2) The following names are hereby placed on the Official List of Specific Names in Zoology:
 - (a) *lesueurii* J.E. Gray, 1850, as published in the binomen *Atlanta lesueurii*, with the endorsement that it is not invalid by reason of being a junior primary homonym of *Atlanta lesueurii* d'Orbigny, 1836;
 - (b) *lesueurii* d'Orbigny, 1836, as published in the binomen *Atlanta lesueurii*.

History of Case 3404

An application to conserve the specific name of the heteropod *Atlanta lesueurii* Souleyet, 1852 (ATLANTIDAE) (correctly attributed to J.E. Gray, 1850, see comments below), originally published as a primary homonym of the pteropod *Atlanta lesueurii* d'Orbigny, 1836 (currently *Limacina lesueurii*), was received from A.W. Janssen (*Xewkija, Gozo, Malta*) and R.R. Seapy (*California State University, Fullerton, CA, U.S.A.*) on 9 January 2007. After correspondence the case was published in BZN 64: 224–226. The title, abstract and keywords of the case were published on the Commission's website. No comments were received on this case.

Decision of the Commission

On 1 September 2008 the members of the Commission were invited to vote on the proposals published in BZN 64: 225. At the close of the voting period on 1 December 2008 the votes were as follows:

Affirmative votes – 19: Alonso-Zarazaga, Bogutskaya, Bouchet, Brothers, Fautin, Grygier, Halliday, Krell, Kullander, Lamas, Lim, Minelli, Pape, Papp, Patterson, Štys, Rosenberg, van Tol and Zhang.

Negative vote – 1: Kottelat.

No vote was received from Ng. Pyle was on leave of absence.

Rosenberg, voting FOR, noted that the application was incorrect in attributing *Atlanta lesueurii* to Souleyet (1852). Souleyet's plates were published in 1846, with captions in the French vernacular. J.E. Gray (1850, *Explanation of the Plates*, p. 101, pl. 241, fig. 5, in M.E. Gray, *Figures of the Molluscous Animals*, vol. 4) latinised and so validated the names. Rosenberg said that if the name is added to the Official List, it should be as *Atlanta lesueurii* J.E. Gray, 1850. This was supported in comments by Bouchet, Brothers, Fautin and Lamas.

Original references

The following are the original references to the names placed on Official Lists and Index by the ruling given in the present Opinion:

lesueurii, *Atlanta*, J.E. Gray, 1850, *Explanation of the plates*, p. 101, in M.E. Gray, *Figures of the molluscous animals*, vol. 4, pl. 241, fig. 5.

lesueurii, *Atlanta*, d'Orbigny, 1836, *Voyage dans l'Amérique méridionale . . .*, vol. 5(3), *Mollusques*, p. 177.

OPINION 2217 (Case 3389)***Heterocarpus gibbosus* Bate, 1888 (Crustacea, Decapoda, PANDALIDAE): replacement of the holotype by a neotype**

Abstract. The holotype of the deep-sea pandalid prawn *Heterocarpus gibbosus* Bate, 1888 is extremely poorly preserved. With this ruling it is replaced with a recently collected neotype from the type locality in the Philippines.

Keywords. Nomenclature; taxonomy; Decapoda; PANDALIDAE; *Heterocarpus gibbosus*; Philippines; pandalid prawn.

Ruling

- (1) Under the plenary power it is hereby ruled that all previous type fixations for the nominal species *gibbosus* Bate, 1888, as published in the binomen *Heterocarpus gibbosus*, are set aside and specimen NHM 2006.1216, Bohol Sea, 'Panglao 2005' at the Natural History Museum, London is designated as the neotype.
- (2) The name *gibbosus* Bate, 1888, as published in the binomen *Heterocarpus gibbosus* and as defined by the neotype designated in (1) above, is hereby placed on the Official List of Specific Names in Zoology.

History of Case 3389

An application was received from X. Li (*Institute of Oceanology, Chinese Academy of Sciences, Qingdao, China*), T.-Y. Chan (*National Taiwan Ocean University, Taiwan*) and P.K.L. Ng (*National University of Singapore, Republic of Singapore*) on 31 May 2006 to replace the poorly preserved holotype of the deep-sea pandalid prawn *Heterocarpus gibbosus* Bate, 1888 with a recently collected neotype from the type locality in the Philippines. After correspondence the case was published in BZN 64: 155–159. The title, abstract and keywords of the case were published on the Commission's website. No comments on this case were received.

Decision of the Commission

On 1 June 2008 the members of the Commission were invited to vote on the proposals published in BZN 64: 158–159. At the close of the voting period on 1 September 2008 the votes were as follows:

Affirmative votes – 15: Alonso-Zarazaga, Bouchet, Brothers, Fautin, Grygier, Halliday, Kottelat, Krell, Kullander, Lamas, Pape, Papp, Rosenberg, van Tol and Zhang.

Negative votes – 2: Bogutskaya and Lim.

No vote was received from Ng. Minelli, Patterson, Pyle and Štys were on leave of absence.

Kullander, voting FOR, said that he fully appreciated that the type specimen appeared to be useless.

Original references

The following is the original reference to the name placed on the Official List by the ruling given in the present Opinion:

gibbosus, *Heterocarpus*, Bate, 1888, *Report on the scientific results of the voyage of H.M.S. Challenger during the years 1873–1876*, vol. 24, part 52, p. 634.

The following is the reference for the description of the neotype:

Li, X., Chan, T.-Y. & Ng, P.K.L. 2007. *Bulletin of Zoological Nomenclature*, **64**(3): 158.

OPINION 2218 (Case 3403)***Mecistocephalus* Newport, 1843 and *Pachymerium* Koch, 1847
(Chilopoda): current usage conserved by designation of
Mecistocephalus punctifrons Newport, 1843 as the type species of
Mecistocephalus Newport, 1843**

Abstract. The Commission has ruled that the usage of the generic names *Mecistocephalus* Newport, 1843 and *Pachymerium* Koch, 1847 for two well-established genera of centipedes is conserved by setting aside type species fixations before the designation by Pocock (1898) of *Mecistocephalus punctifrons* Newport, 1843, as the type species of *Mecistocephalus*.

Keywords. Nomenclature; taxonomy; type species; Chilopoda; *Mecistocephalus*; *Pachymerium*; *Geophilus ferrugineus*; *Mecistocephalus punctifrons*; geophilomorph centipedes.

Ruling

- (1) Under the plenary power it is hereby ruled that all previous fixations of type species for the nominal genus *Mecistocephalus* Newport, 1843 before the designation by Pocock (1898) of *Mecistocephalus punctifrons* Newport, 1843 as the type species, are set aside.
- (2) The following names are hereby placed on the Official List of Generic Names in Zoology:
 - (a) *Mecistocephalus* Newport, 1843 (gender: masculine), type species *Mecistocephalus punctifrons* Newport, 1843, as ruled in (1) above;
 - (b) *Pachymerium* Koch, 1847 (gender: masculine), type species *Geophilus ferrugineus* Koch, 1835, by monotypy.
- (3) The following names are hereby placed on the Official List of Specific Names in Zoology:
 - (a) *punctifrons* Newport, 1843, as published in the binomen *Mecistocephalus punctifrons* (specific name of the type species of *Mecistocephalus* Newport, 1843);
 - (b) *ferrugineus* Koch, 1835, as published in the binomen *Geophilus ferrugineus* (specific name of the type species of *Pachymerium* Koch, 1847).

History of Case 3403

An application to conserve *Mecistocephalus punctifrons* Newport, 1843 as the type species of the genus *Mecistocephalus* Newport, 1843, by suppression of the designation of *Geophilus ferrugineus* Koch, 1835 as the type species of this genus was received from L. Bonato and A. Minelli (*University of Padova, Italy*) on 15 December 2006. After correspondence the case was published in BZN 64: 166–173. The title, abstract and keywords of the case were published on the Commission's website. No comments were received on this case.

Decision of the Commission

On 1 June 2008 the members of the Commission were invited to vote on the proposals published in BZN 64: 169. At the close of the voting period on 1 September 2008 the votes were as follows:

Affirmative votes – 16: Alonso-Zarazaga, Bouchet, Brothers, Fautin, Grygier, Halliday, Kottelat, Krell, Kullander, Lamas, Lim, Pape, Papp, Rosenberg, van Tol and Zhang.

Negative vote – 1: Bogutskaya.

No vote was received from Ng. Minelli, Patterson, Pyle and Štys were on leave of absence.

Original references

The following are the original references to the names placed on Official Lists by the ruling given in the present Opinion:

Mecistocephalus Newport, 1843, *Proceedings of the Zoological Society of London*, **10**: 178.

Pachymerium Koch, 1847, in Herrich-Schäffer, L. (Ed.), *Kritische Revision der Insecten-faune Deutschlands*, vol. 3, p. 85.

punctifrons, *Mecistocephalus*, Newport, 1843, *Proceedings of the Zoological Society of London*, **10**: 179.

ferrugineus, *Geophilus*, Koch, 1835, *Deutschlands Crustaceen, Myriapoden und Arachniden*, heft 3, tab. 2, unnumbered page.

The following is the reference for the designation of *Geophilus punctifrons* Newport, 1843 as the type species of the nominal genus *Mecistocephalus* Newport, 1843:

Pocock, R.I. 1898. Report on the centipedes and millipedes obtained by Dr A. Willey in the Loyalty Islands, New Britain, and elsewhere. Pp. 59–74 in Willey, A. (Ed.), *Zoological Results based on material from New Britain, New Guinea, Loyalty Isles and elsewhere*, vol. 1, p. 63.

OPINION 2219 (Case 3408)***Geophilus holstii* Pocock, 1895 (currently *Arrup holstii*; Chilopoda, MECISTOCEPHALIDAE): replacement of the holotype by a neotype**

Abstract. The holotype of *Geophilus holstii* Pocock, 1895 (currently *Arrup holstii*), a centipede from Japan, is unidentifiable, thus with this ruling it is replaced by a neotype.

Keywords. Nomenclature; taxonomy; Chilopoda; Geophilomorpha; MECISTOCEPHALIDAE; *Arrup*; *Arrup holstii*; centipede; Japan.

Ruling

- (1) Under the plenary power it is hereby ruled that the existing holotype of *Geophilus holstii* Pocock, 1895 is set aside and the male specimen as specified in Uliana et al. (2007), with collecting data Hon-noo, Mobara City, Chiba Pref., Honshu, Japan 3. XII 1997, K. Ishii leg., and accessed in the National Science Museum, Tokyo, Japan, is designated as neotype.
- (2) The name *holstii* Pocock, 1895, as published in the binomen *Geophilus holstii* and as defined by the neotype designated in (1) above, is hereby placed on the Official List of Specific Names in Zoology.

History of Case 3408

An application to replace the existing unidentifiable holotype of *Geophilus holstii* Pocock, 1895 (currently *Arrup holstii*) by a neotype was received from M. Uliana, L. Bonato and A. Minelli (*University of Padova, Italy*) on 15 January 2007. After correspondence the case was published in BZN 64: 227–229. The title, abstract and keywords of the case were published on the Commission's website. No comments were received on this case.

Decision of the Commission

On 1 September 2008 the members of the Commission were invited to vote on the proposals published in BZN 64: 229. At the close of the voting period on 1 December 2008 the votes were as follows:

Affirmative votes – 20: Alonso-Zarazaga, Bogutskaya, Bouchet, Brothers, Fautin, Grygier, Halliday, Kottelat, Krell, Kullander, Lamas, Lim, Minelli, Pape, Papp, Patterson, Štys, Rosenberg, van Tol and Zhang.

Negative vote – none.

No vote was received from Ng. Pyle was on leave of absence.

Original references

The following is the original reference to the name placed on an Official List by the ruling given in the present Opinion:

holstii, *Geophilus*, Pocock, 1895, *Annals and Magazine of Natural History*, (6)15: 352.

The following is the reference for the description of the neotype:

Uliana, M., Bonato, L. & Minelli, A. 2007, *Zootaxa*, 1396: 10.

OPINION 2220 (Case 3392)

***Hemerobius elegans* Stephens, 1836 (currently *Sympherobius elegans*) and *Hemerobius elegans* Guérin-Méneville, 1844 (currently *Vieira elegans*) (Insecta, Neuroptera): specific names conserved**

Abstract. The specific names *Hemerobius elegans* Stephens, 1836 (currently *Sympherobius elegans*), for a European brown lacewing (HEMEROBIIDAE) and *Hemerobius elegans* Guérin-Méneville, 1844 (currently *Vieira elegans*), for a Neotropical green lacewing (CHRYSOPIIDAE) are hereby conserved by ruling that *Hemerobius elegans* Guérin-Méneville, 1844 is not invalid by reason of being a junior primary homonym of *Hemerobius elegans* Stephens, 1836. Both names are in current use but are placed in different families and unlikely to ever be treated as congeneric in the future.

Keywords. Nomenclature; taxonomy; CHRYSOPIIDAE; HEMEROBIIDAE; *Vieira elegans*; *Sympherobius elegans*; green lacewings; brown lacewings; Costa Rica; Suriname; Brazil; Europe.

Ruling

- (1) Under the plenary power it is hereby ruled that the name *elegans* Guérin-Méneville, 1844, as published in the binomen *Hemerobius elegans*, is not invalid by reason of being a junior primary homonym of *elegans* Stephens, 1836, as published in the binomen *Hemerobius elegans*;
- (2) The following names are hereby placed on the Official List of Specific Names in Zoology:
 - (a) *elegans* Stephens, 1836, as published in the binomen *Hemerobius elegans*;
 - (b) *elegans* Guérin-Méneville, 1844, as published in the binomen *Hemerobius elegans*, with the endorsement that it is not invalid by reason of being a junior primary homonym of *elegans* Stephens, 1836, as published in the binomen *Hemerobius elegans*, as ruled in (1) above.

History of Case 3392

An application to conserve the specific names *Hemerobius elegans* Stephens, 1836 (currently *Sympherobius elegans*) (HEMEROBIIDAE) and *Hemerobius elegans* Guérin-Méneville, 1844 (currently *Vieira elegans*) (CHRYSOPIIDAE) was received from J.D. Oswald (*Texas A&M University, TX, U.S.A.*) on 14 June 2006. After correspondence the case was published in BZN 64: 174–177. The title, abstract and keywords of the case were published on the Commission's website. One supportive comment was published in BZN 65: 49–50.

Decision of the Commission

On 1 June 2008 the members of the Commission were invited to vote on the proposals published in BZN 64: 175. At the close of the voting period on 1 September 2008 the votes were as follows:

Affirmative votes – 12: Alonso-Zarazaga, Bogutskaya, Bouchet, Brothers, Halliday, Krell, Kullander, Lamas, Lim, Pape, Papp and Zhang.

Negative votes – 4: Grygier, Kottelat, Rosenberg and van Tol.

Fautin abstained.

No vote was received from Ng. Minelli, Patterson, Pyle and Štys were on leave of absence.

Voting AGAINST, Grygier commented that the junior name appears to hold no special importance. Also voting AGAINST, Kottelat said that he failed to see how the change of the name of a species said to be rare and cited six times in the last 50 years can threaten stability of nomenclature. Kullander, voting FOR, said that this was a relevant case for the Commission and that he particularly appreciates applications that do more than attempt to preserve favourite junior synonyms. Rosenberg, voting AGAINST, commented that *Hemerobius elegans* Guérin-Méneville, 1844 does not have enough usage to warrant conservation.

Original references

The following are the original references to the names placed on Official Lists by the ruling given in the present Opinion:

elegans, *Hemerobius*, Stephens, 1836, *Illustrations of British entomology; or, a synopsis of indigenous insects: containing their generic and specific distinctions; with an account of their metamorphoses, times of appearance, localities, food, economy . . . Mandibulata*, vol. 6, p. 113.

elegans, *Hemerobius*, Guérin-Méneville, 1844, *Iconographie du règne animal de G. Cuvier, ou représentation d'après nature de l'une des espèces les plus remarquables, et souvent non encore figurées, de chaque genre d'animaux. Insectes*, p. 388.

OPINION 2221 (Case 3388)

***Buprestis angustula* Illiger, 1803 (currently *Agrilus angustulus*, Insecta, Coleoptera): specific name given precedence over the specific name *Buprestis pavidus* Fabricius, 1793**

Abstract. The specific name *Buprestis angustula* Illiger, 1803 (currently *Agrilus angustulus*) for a widely distributed species of jewel beetle (family BUPRESTIDAE) has been given precedence over its unused senior synonym *Agrilus pavidus* (Fabricius, 1793).

Keywords. Nomenclature; taxonomy; Coleoptera; BUPRESTIDAE; *Agrilus angustulus*; *Agrilus pavidus*; buprestids; jewel beetles; Europe; Asia.

Ruling

- (1) Under the plenary power it is hereby ruled that the specific name *angustula* Illiger, 1803, as published in the binomen *Buprestis angustula*, is given precedence over the name *pavida* Fabricius, 1793, as published in the binomen *Buprestis pavidus*, whenever the two are considered to be synonyms.
- (2) The following names are placed on the Official List of Specific Names in Zoology:
 - (a) *angustula* Illiger, 1803, as published in the binomen *Buprestis angustula*, with the endorsement that it is to be given precedence over the name *pavida* Fabricius, 1793, as published in the binomen *Buprestis pavidus*, whenever the two are considered to be synonyms;
 - (b) *pavida* Fabricius, 1793, as published in the binomen *Buprestis pavidus*, with the endorsement that it is not to be given precedence over the name *angustula* Illiger, 1803, as published in the binomen *Buprestis angustula*, whenever the two are considered to be synonyms.

History of Case 3388

An application to conserve the specific name *Agrilus angustulus* (Illiger, 1803) for a widely distributed species of jewel beetle (family BUPRESTIDAE) by giving it precedence over its unused senior synonym *Agrilus pavidus* (Fabricius, 1793) was received from E. Jendek (*Institute of Zoology, Slovak Academy of Sciences, Slovakia*) on 30 May 2006. After correspondence the case was published in BZN 64: 178–181. The title, abstract and keywords of the case were published on the Commission's website. Four supportive comments were published in BZN 64: 191–192 and in BZN 64: 261.

Decision of the Commission

On 1 June 2008 the members of the Commission were invited to vote on the proposals published in BZN 64: 179. At the close of the voting period on 1 September 2008 the votes were as follows:

Affirmative votes – 13: Alonso-Zarazaga, Bogutskaya, Bouchet, Brothers, Grygier, Halliday, Kottelat, Krell, Lamas, Papp, Rosenberg, van Tol and Zhang.

Negative votes – 3: Lim, Kullander and Pape.

Fautin abstained.

No vote was received from Ng. Minelli, Patterson, Pyle and Štys were on leave of absence.

Original references

The following are the original references to the names placed on Official Lists by the ruling given in the present Opinion:

angustula, *Buprestis*, Illiger, 1803, *Magazin für Insektenkunde*, 2: 240.

pavida, *Buprestis*, Fabricius, 1793, *Entomologia systematica emendata et aucta. Secundum classes, ordines, genera, species adjectis synonymis, locis, observationibus, descriptionibus*, vol. 1, part 2, p. 214.

OPINION 2222 (Case 3335)***Trachys* Fabricius, 1801 (Insecta, Coleoptera): masculine gender of the genus fixed**

Abstract. The Commission has ruled that the stem of the genus *Trachys* is *Trache-*, and the name *Trachys* Fabricius, 1801 should be treated as of masculine gender. The family name TRACHISIDAE Laporte, 1835 is ruled to be an incorrect original spelling and is placed on the Official Index of Rejected and Invalid Family-Group Names in Zoology. The family-group names TRACHYINI Gavoy, 1901, TRACHYDINI Reitter, 1911, TRACHYNI Kerremans, 1893 and TRACHYNINI Kraatz, 1869 are placed on the Official Index of Rejected and Invalid Family-Group Names in Zoology as incorrectly derived from *Trachys* Fabricius, 1801.

Keywords. Nomenclature; taxonomy; Coleoptera; BUPRESTIDAE; *Trachys*; *Trachys minutus*; gender; leaf-mining jewel beetle.

Ruling

- (1) Under the plenary power it is hereby ruled that:
 - (a) the stem of the genus *Trachys* is *Trache-*;
 - (b) the gender of the name *Trachys* Fabricius, 1801 is masculine.
- (2) The name *Trachys* Fabricius, 1801 (gender: masculine), type species by subsequent designation by Westwood (1838) *Buprestis minuta* Linnaeus, is hereby placed on the Official List of Generic Names in Zoology.
- (3) The name *minuta* Linnaeus, 1758, as published in the binomen *Buprestis minuta* (specific name of the type species of *Trachys* Fabricius, 1801) is hereby placed on the Official List of Specific Names in Zoology.
- (4) The name TRACHEIDAE Laporte, 1835 (type genus *Trachys* Fabricius, 1801), a corrected original spelling from TRACHISIDAE, as ruled in (1) above, is hereby placed on the Official List of Family-Group Names in Zoology.
- (5) The following names, incorrectly derived from *Trachys* Fabricius, 1801, are hereby placed on the Official Index of Rejected and Invalid Family-Group Names in Zoology:
 - (a) TRACHISIDAE Laporte, 1835 (an incorrect original spelling);
 - (b) TRACHYINI Gavoy, 1901;
 - (c) TRACHYDINI Reitter, 1911;
 - (d) TRACHYNI Kerremans, 1893;
 - (e) TRACHYNINI Kraatz, 1869.

History of Case 3335

An application to validate the feminine gender of the generic name *Trachys* Fabricius, 1801 for a common and widespread leaf-mining jewel beetle and the form of derivation of the family-group names based on *Trachys* was received from S. Bílý (National Museum, Prague, Czech Republic) and V. Kubáň (Moravian Museum, Brno, Czech Republic) on 29 November 2004. After correspondence the case was published

in BZN 63: 172–176. The title, abstract and keywords of the case were published on the Commission's website. On 1 June 2007 the members of the Commission were invited to vote on the proposals set out in BZN 63: 173–174. After discussion between the Commissioners and the Executive Secretary, it was agreed that the voting on Case 3335 should be postponed. A supportive comment on this case was published in BZN 64: 66. Adverse comments on this case were published in BZN 63: 273–274; 64: 64–66, 187–191 (the latter comment with an alternative set of proposals).

Decision of the Commission

On 1 June 2008 the members of the Commission were invited to vote on the proposals published in BZN 63: 173–174 and BZN 64: 190. At the close of the voting period on 1 September 2008 the votes were as follows:

Proposals in BZN 63: 173–174

Affirmative vote – 1: Lim.

Negative votes – 16: Alonso-Zarazaga, Bogutskaya, Bouchet, Brothers, Fautin, Grygier, Halliday, Kottelat, Krell, Kullander, Lamas, Pape, Papp, Rosenberg, van Tol and Zhang.

No vote was received from Ng. Minelli, Patterson, Pyle and Štys were on leave of absence.

Proposals in BZN 64: 190

Affirmative votes – 15: Alonso-Zarazaga, Bogutskaya, Brothers, Fautin, Grygier, Halliday, Kottelat, Krell, Kullander, Lamas, Pape, Papp, Rosenberg, van Tol and Zhang.

Negative vote – 1: Lim.

Bouchet split his vote, voting FOR the proposals BZN 64: 190 with the exception of proposal 5 (b)–(e).

No vote was received from Ng. Minelli, Patterson, Pyle and Štys were on leave of absence.

Although Bouchet voted FOR Proposals 1–5 (a), he objected to placing the following names on the Official Lists: TRACHYINI Gavoy, 1901; TRACHYDINI Reitter, 1911; TRACHYNI Kerremans, 1893; TRACHYNINI Kraatz, 1869. He said that under Article 33.3 (Incorrect subsequent spellings) and Article 36.1 (Principle of Coordination), these names are mere variations of TRACHEIDAE Laporte, 1835. They do not have their own nomenclatural availability; they do not have their own author and date; they cannot be treated as separate names.

In the course of the discussion, Kerzhner commented that *Trachys* was proposed by Fabricius as feminine with genitive *trachydis* as different from the Greek adjective *trachys*, masculine with the genitive *tracheos* (*tracheus*); it was hardly probable that Fabricius had in mind this adjective meaning rough for these smooth, shiny beetles. Kerzhner said he did not think that Fabricius, who wrote 'in an acceptable Latin', simply erred. Of 33 Fabrician generic names in Hemiptera, 16 are identical to Latin geographic names and others probably originate from such names with the change of a few letters (e.g. *Miris* from *Myris*). Kerzhner said it is possible that Fabricius formed his *Trachys* from *Trachis* (an ancient Greek city) and considered it feminine and with genitive ending in *-ydis* as in the words *chlamys* or *emys*. Kerzhner commented further that the current masculine usage of *Trachys* is about three-fold greater than feminine usage. However, he did not see the need for the Commission to address all 430 other names ending in *-ys*.

Original references

The following are the original references to the names placed on Official Lists and Indexes by the ruling given in the present Opinion:

minuta, *Buprestis*, Linnaeus, 1758, *Systema Naturae*, Ed. 10, p. 410.

TRACHISIDAE Laporte, 1835, *Revue Entomologique*, **3**: 166.

TRACHYDINI Reitter, 1911, *Fauna Germanica. Die Käfer des Deutschen Reiches. Nach der analytische Methode bearbeitet*, vol. 3, p. 180.

TRACHYINI Gavoy, 1901, *Bulletin de la Société d'Études Scientifiques de l' Aude*, **12**: 113.

TRACHYNI Kerremans, 1893, *Annales de la Société Entomologique de Belgique*, **37**: 121.

TRACHYNINI Kraatz, 1869, *Verzeichniss der Käfer Deutschlands*, p. 36.

Trachys Fabricius, 1801, *Systema Eleutheratorum secundum ordines, genera, species: adjectis synonymis, locis, observationibus, descriptionibus. Tomus II*, p. 218.

OPINION 2223 (Case 3345)**DENDROBATIDAE Cope, 1865 (1850) (Amphibia, Anura): family-group name conserved**

Abstract. The widely used family-group name DENDROBATIDAE Cope, 1865 (1850) is conserved by giving it precedence over the senior synonym PHYLLOBATIDAE Fitzinger, 1843 whenever the two are considered synonyms. As a further protection of the family name of this group of Neotropical frogs, the generic name *Hysaplesia* Boie in Schlegel, 1826, considered by some authors as a senior synonym of the generic name *Dendrobates* Wagler, 1830, is hereby suppressed.

Keywords. Nomenclature; taxonomy; Amphibia; DENDROBATIDAE; *Dendrobates*; *Phyllobates*; *Hysaplesia*; *Dendrobates tinctorius*; *Phyllobates bicolor*; poison arrow frogs; poison dart frogs.

Ruling

- (1) Under the plenary power it is hereby ruled that:
 - (a) the generic name *Hysaplesia* Boie in Schlegel, 1826 (gender: feminine), type species '*Hyla punctata* Daudin' = *Calamita punctatus* Schneider, 1799 by subsequent designation of Stejneger, 1937 is suppressed for the purposes of the Principle of Priority but not for those of the Principle of Homonymy;
 - (b) the family-group name DENDROBATIDAE Cope, 1865 (1850) is given precedence over the family-group name PHYLLOBATIDAE Fitzinger, 1843 whenever the two are regarded as synonyms.
- (2) The following names are hereby placed on the Official List of Generic Names in Zoology:
 - (a) *Dendrobates* Wagler, 1830 (gender: masculine), type species *Rana tinctoria* Cuvier, 1797, by subsequent designation by Duméril & Bibron (1841);
 - (b) *Phyllobates* Bibron, 1840 (gender: masculine), type species *Phyllobates bicolor* Bibron in de la Sagra, 1840, by monotypy.
- (3) The following names are hereby placed on the Official List of Specific Names in Zoology:
 - (a) *tinctoria* Cuvier, 1797, as published in the binomen *Rana tinctoria* (specific name of the type species of *Dendrobates* Wagler, 1830);
 - (b) *bicolor* Bibron, 1840, as published in the binomen *Phyllobates bicolor* (specific name of the type species of *Phyllobates* Bibron in de la Sagra, 1840).
- (4) The following names are hereby placed on the Official List of Family-Group Names in Zoology:
 - (a) DENDROBATIDAE Cope, 1865 (1850) (type genus *Dendrobates* Wagler, 1830) with an endorsement that it is to be given precedence over PHYLLOBATIDAE Fitzinger, 1843 (type genus *Phyllobates* Bibron in de la Sagra, 1840) whenever the two names are considered synonyms;

- (b) PHYLLOBATIDAE Fitzinger, 1843 (type genus *Phyllobates* Bibron in de la Sagra, 1840) with an endorsement that it is not to be given priority over DENDROBATIDAE Cope, 1865 (1850) whenever the two names are considered synonyms.
- (5) The genus *Hysaplesia* Boie in Schlegel, 1826 (gender: feminine), type species '*Hyla punctata* Daudin' = *Calamita punctatus* Schneider, 1799, by subsequent designation by Stejneger (1937), is hereby placed on the Official Index of Rejected and Invalid Generic Names in Zoology.

History of Case 3345

An application to conserve the widely used family-group name DENDROBATIDAE Cope, 1865 (1850) for a group of Neotropical frogs by giving it precedence over the senior synonym PHYLLOBATIDAE Fitzinger, 1843, was received from J.M. Savage (*San Diego State University, CA, U.S.A.*), C.W. Myers, D.R. Frost (both *American Museum of Natural History, NY, U.S.A.*) and T. Grant (*Pontifica Universidade Católica do Rio Grande do Sul, Brazil*) on 4 February 2005. After correspondence the case was published in BZN 64: 255–260. The title, abstract and keywords of the case were published on the Commission's website. No comments were received on this case.

Decision of the Commission

On 1 September 2008 the members of the Commission were invited to vote on the proposals published in BZN 64: 258–259. At the close of the voting period on 1 December 2008 the votes were as follows:

Affirmative votes – 17: Alonso-Zarazaga, Bogutskaya, Bouchet, Brothers, Fautin, Grygier, Halliday, Krell, Kullander, Lamas, Minelli, Pape, Papp, Patterson, Štys, Rosenberg and Zhang.

Negative votes – 2: Lim and van Tol.

Kottelat abstained.

No vote was received from Ng. Pyle was on leave of absence.

Bouchet, voting FOR, cautioned that the name of the work containing the description of *Phyllobates* should be cited as de la Sagra, and not Sagra, as it was cited in the original application. He also mentioned, as he has done elsewhere, that he dislikes conditional reversals of precedence, as they are a potential source of nomenclatural instability. However, he voted in favour of the proposal as there was no alternative offered to conserve the name DENDROBATIDAE. He also suggested that to give credit where it is due, it would have been diplomatic for the authors to invite Dubois to join their application. Failing that, upon receipt of Case 3345, the Secretariat might have considered reviving Dubois' initial application of 1982, which was legitimate and well-founded, though never went to vote due to a perceived shortcoming of one part by a single Commissioner. Voting FOR, Brothers commented that the name *Hylaplesia* (incorrect subsequent spelling of *Hysaplesia*) should probably also be placed on the Index (with the appropriate wording and attributions). Also voting FOR, Štys pointed out that DENDROBATIDAE is one of the most intensely studied families of the anurans. Consequently, he was unpleasantly surprised that no one in the batrachological community cared to comment about its correct name.

Original references

The following are the original references to the names placed on Official Lists and Index by the ruling given in the present Opinion:

bicolor, *Phyllobates*, Bibron, 1840, in Cocteau, J.T. & Bibron, G. Reptiles, in de la Sagra, R. (Ed.), *Historia fisica, politica y natural de la Isla de Cuba*. Part 2, *Historia natural*, vol. 8, *Atlas de Zoologia*, pl. 29b.

Dendrobates Wagler, 1830, *Natürliches System der Amphibien mit vorangehender Classification der Säugthiere und Vögel. Ein Beitrag zur vergleichenden Zoologie*, vi, p. 202.

DENDROBATIDAE Cope, 1865 (1850), *Natural History Review*, N.S., 5: 100.

Hysaplesia Boie in Schlegel, 1826, *Bulletin des Sciences Naturelles et de Géologie* (Paris), 9(2): 239.

Phyllobates Bibron, 1840, in Cocteau, J.T. & Bibron, G. Reptiles, in de la Sagra, R. (Ed.), *Historia fisica, politica y natural de la Isla de Cuba*. Part 2, *Historia natural*, vol. 8, *Atlas de Zoologia*, pl. 29b.

PHYLLOBATIDAE Fitzinger, 1843, *Systema Reptilium. Fasciculus primus. Amblyglossae*, p. 32.

tinctoria, *Rana*, Cuvier, 1797, *Tableau élémentaire de l'histoire naturelle des animaux*, p. 295.

Information and Instructions for Authors

The following notes are primarily for those preparing applications; other authors should comply with the relevant sections. Applications should be prepared in the format of recent parts of the *Bulletin*; manuscripts not prepared in accordance with these guidelines may be returned. For more detailed instructions see http://www.iczn.org/Instructions_for_authors.htm.

General. Applications are requests to the Commission to set aside or modify the Code's provisions as they relate to a particular name or group of names when this appears to be in the interest of stability of nomenclature. Authors submitting cases should regard themselves as acting on behalf of the zoological community and the Commission will treat applications on this basis. Applicants should discuss their cases with other workers in the same field before submitting applications, so that they are aware of any wider implications and the likely reactions of other zoologists.

Text. Formatted with double spacing, this should consist of numbered paragraphs setting out the details of the case and leading to a final paragraph of formal proposals. Text references should give dates and page numbers in parentheses, e.g. 'Daudin (1800, p. 49) described ...'. If the plenary power of the Commission is sought, this necessity should be clearly explained (with reference to the relevant Articles of the Code).

References. These should give all authors of a publication. Where possible, ten or more reasonably recent references should be given illustrating the usage of names that are to be conserved or given precedence over older names. The title of periodicals should be in full and in italics; number of volumes, parts, etc. should be in Arabic figures, separated by a colon from the page numbers. Book titles should be in italics and followed by the number of pages and plates, the publisher and place of publication.

Submission of application: Applications should be submitted electronically preferably by email (within the message or as an attachment) to 'iczn@nhm.ac.uk'. When submitted by post, one copy should be sent to: The Executive Secretary, International Commission on Zoological Nomenclature, c/o The Natural History Museum, Cromwell Road, London SW7 5BD, U.K. The typescript should be accompanied by a disk with copy in IBM PC compatible format (disks and attachments to be in Word, rtf or ASCII text). It would also be helpful if applications were accompanied by photocopies of relevant pages of the main references where this is possible.

The Commission's Secretariat is very willing to advise on all aspects of the formulation of an application.

Illustrations

If a neotype designation is requested it is strongly advisable to submit an appropriate illustration in TIFF format of sufficiently high resolution for printing. Hardcopy figures will be accepted, at discretion of the Editor. It is desirable to illustrate both specimen and its label, with a scale bar. Furthermore, we are willing to consider illustrations of the organisms under discussion contributed simply to add visual interest to the applications. Line drawings should be high-resolution TIFFs at 600–1200 dpi; photographs may be 300–400 dpi. If the files are difficult to send electronically, LZW compression of TIFF files allows recovery of original file quality. Please note that JPG format often introduces artefacts in printing, thus we are not able to accept JPGs.

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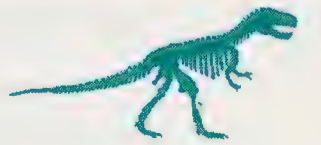
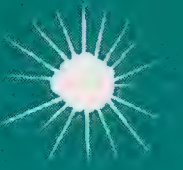
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Cover image: *Portunus pelagicus* (L.), the blue swimming crab native to Indo-Pacific waters, Plate 49 from Zoological drawings by F.L. Bauer (© The Natural History Museum, London, 1997, housed in the collection of the General and Zoology Library).

BULLETIN OF ZOOLOGICAL NOMENCLATURE

Volume 66, part 2 (pp. 107–202)

30 June 2009

Notices

(1) Applications and correspondence relating to applications to the Commission should be sent to the Executive Secretary at the address given on the inside of the front cover and on the Commission website. English is the official language of the *Bulletin*. Please take careful note of instructions to authors (present in a one- or two-page form in each volume and available online at <http://www.iczn.org/guidelines.html>) as incorrectly formatted applications will be returned to authors for revision. The Commission's Secretariat will answer general nomenclatural (as opposed to purely taxonomic) enquiries and assist with the formulation of applications and, as far as it can, check the main nomenclatural references in applications. Correspondence should be sent by e-mail to 'iczn@nhm.ac.uk' where possible.

(2) The Commission votes on applications eight months after they have been published, although this period is normally extended to enable comments to be submitted. Comments for publication relating to applications (either in support or against, or offering alternative solutions) should be submitted as soon as possible. Comments may be edited (see instructions for submission of comments at http://www.iczn.org/Instructions_for_comments.html).

(3) Requests for help and advice on the Code can be made direct to the Commission and other interested parties via the Internet. Membership of the Commission's Discussion List is free of charge. You can subscribe and find out more about the list at <http://list.afriherp.org/mailman/listinfo/iczn-list>.

(4) The Commission also welcomes the submission of general-interest articles on nomenclatural themes or nomenclatural notes on particular issues. These may deal with taxonomy, but should be mainly nomenclatural in content. Articles and notes should be sent to the Executive Secretary.

New applications to the Commission

The following new applications have been received since the last issue of the *Bulletin* (volume 66, part 1, 31 March 2009) went to press. Under Article 82 of the Code, the existing usage of names in the applications is to be maintained until the Commission's rulings on the applications (the Opinions) have been published.

CASE 3489: *Chrysomela elongata* Suffrian, 1851 (currently *Oreina elongata*; Insecta, Coleoptera): proposed conservation. H. Silfverberg.

CASE 3490: *Lacerta agama* Linnaeus, 1758 (Squamata, AGAMIDAE): proposed conservation of usage by designation of a neotype. P. Wagner & W. Böhme.

CASE 3491: *Podargus cornutus* Temminck, 1822 (Aves, Caprimulgiformes, PODARGIDAE): proposed conservation of usage of the specific name. N. Cleere, E.C. Dickinson, J.-F. Voisin & C. Voisin.

CASE 3492: *Heliconius tristero* Brower, 1996 and *Heliconius melpomene mocoa* Brower, 1996 (Lepidoptera, NYMPHALIDAE): proposed precedence over *Heliconius melpomene bellula*. A.V.Z. Brower.

CASE 3493: *Haliplanella* Treadwell, 1943 (Polychaeta): proposed suppression in favour of *Haliplanella* Hand, 1956 (Anthozoa). D.G. Fautin, C. Hand (deceased) & M. Daly.

CASE 3494: *Atlanta inflata* Gray, 1850 (Mollusca, Gastropoda, PTEROTRACHEOIDEA, ATLANTIDAE): proposed conservation of the specific name. A.W. Janssen & R.R. Seapy.

CASE 3495: *Chionobas chryxus* Doubleday, 1849 (currently *Oeneis chryxus*) (Insecta, Lepidoptera, NYMPHALIDAE): proposed conservation of usage. J.A. Scott.

Election of six new Commissioners

The Commission announced in September 2007 (BZN 64: 138) that there were vacancies on the Commission and invited nominations for potential candidates. Over the past year, following the rules laid out in the Constitution and Bylaws, the Commission has studied all nominations received and deliberated over the ballot for six new Commissioners.

The Secretariat is very pleased to announce that this process is now complete and that the following people, representing a range of scientific expertise and geographical regions, have been elected to the Commission, effective as of 19 May 2009:

Dr Alberto Ballerio (Italy; Coleoptera)
Dr Mark S. Harvey (Australia; Arachnida)
Prof. Jun-ichi Kojima (Japan; Hymenoptera)
Dr Judith E. Winston (USA; Bryozoa)
Dr Douglas Yanega (USA; Entomology)
Prof. Hongzhang Zhou (China; Coleoptera)

Short biographies are available on the ICZN website: http://www.iczn.org/Election_new_Commissioners_May_2009.html

Registration of scientific names of plants, fungi, bacteria, cultivated plants, and animals: Approaches and experiences across disciplines

David L. Hawksworth

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Introduction

On 29 November 2007, the Linnean Society of London hosted its third systematics debate on the topic ‘Should the registration of new names of organisms be compulsory?’. Immediately prior to the debate itself, five presentations were made on the approaches to, and experiences in, the registration of scientific names in different groups of organisms whose nomenclature is governed by four separate internationally mandated *Codes* of nomenclature. Summaries of these presentations are provided here, along with a synopsis of the debate itself.

The concept of some formal registration process for names of organisms is not new. It was proposed for botanical groups as far back as 1954, but rejected. After an independent *Code* for bacteria had been developed, from 1980 publication of bacterial names was effected only by publication in, or listing in, a single journal. In 1987, a special committee was established by the International Botanical Congress in Berlin to examine how registration might be implemented; the proposals made were adopted at the subsequent congress in Tokyo in 1993 and a trial system established with a view to implementation at the St. Louis congress in 1999 – but, that congress rejected the proposals. However, in the interim, registration had been included as mandatory in the ‘Draft BioCode: the prospective international rules for the scientific names of organisms’ prepared by representatives of all five *Codes* under the auspices of the IUBS/IUMS International Committee on Bionomenclature (ICB) in 1996. In 2004, MycoBank, a voluntary web-based system for new fungal names (and additional information) went live; proposals to make that mandatory for fungi are now in preparation for discussion at the International Mycological Congress in Edinburgh in 2010. For names of animals, the development of ZooBank was first proposed in 2005.

In considering the registration issue today for particular groups of organisms, it is prudent to be cognizant of the experiences and plans of those working with others. The reports presented here aim to provide a background to the current discussions as to the desirability of the compulsory registration of newly proposed scientific names for animals and fungi in particular.

Registration of names: the botanical experience

Werner Greuter

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Definition

‘Registration of names is the process of entering names into a register whereby they acquire a special quality (whereas indexing: the listing of names with a defined set of qualities but without adding to their status)’ (Greuter, 1986). From the beginning, registration has been envisioned as being: (a) non-censorial; (b) generally available and free of cost, at the very least for the authors of names; (c) based on a reliably functioning, preferably decentralised system tested beforehand; and (d), if at all possible, financially self-supporting in the long run.

Rationale

- Any author who publishes a new name wants to make it available and generally known. The onus of doing the needful has always been placed on the author. In this process, registering a name is a small and easy supplementary step, very much in the authors’ own interest.
- It is in the publishers’ interest to ‘pamper’ their authors. Publishers may be expected to be cooperative, especially when their authorship includes significant taxonomic content.
- It is in the users’ interest to be informed quickly, completely and reliably on any and all nomenclatural novelties that are published. Users are unwilling, however, to pay for that service, so it must be free of charge.
- Indexers, where they exist, will see their task alleviated by registration (the active hunt for items to be indexed being replaced by proffered data), and will see the value of their product increased (being now exhaustively complete by definition).

Concise history

1985: At the request of IUBS (International Union of Biological Sciences) and ICSEB (International Congress on Systematic and Evolutionary Biology), the General Committee on Botanical Nomenclature appointed a Committee on Registration of Plant Names, with five members, to report to the Berlin Congress.

1986: The Committee’s report and proposals appeared in *Taxon*. Mandatory registration was recommended from an unspecified date later than the next subsequent Congress in Tokyo in 1993 (Greuter, 1986).

1987: At the Berlin Congress there was a long and lively debate on the independently proposed issues of registration of names and journals. A Special Committee to look into both issues was approved (Greuter et al., 1989).

1991: The Special Committee on Registration, with two subcommittees, met at Kew in February. The idea of journal registration was abandoned, and concrete steps

toward the mandatory registration of new names were supported and a set of proposals was published in *Taxon* (Faegri, 1991).

1993: Together with the NCU (Names in Current Use) proposals, registration became the principal issue at the Tokyo Congress. By a sweeping majority, the principle of mandatory registration of new names was written into the *Code*, to become effective in 2000 after a trial run and subject to approval by the subsequent congress in 1999. The IAPT (International Association for Plant Taxonomy) accepted responsibility for setting up the required structures and procedures (Greuter et al., 1994).

1998: After thorough preparations, the IAPT Registration trial started officially (Borgen et al., 1997) and went online immediately on 5 January with the first 91 entries. At a workshop on 'Removing the Taxonomic Impediment', in February, the 'Darwin Declaration' (Environment Australia, 1998) was formulated. The declaration was later approved by the Conference of Parties to the Convention on Biological Diversity. Among other things, it recommended that 'Institutions/Individuals should support and encourage a system for the registering of newly proposed names of organisms ...'. Proposals to implement the mandatory registration of new plant names were published in *Taxon* (Borgen et al., 1998).

1999: By mid-April, 207 journals had signed a covenant with the IAPT, by which they became accredited with the registration system (meaning that they agreed to care for the registration of new plant names published in them, on behalf of their authors). National registration centres in 38 countries had been set up (Raab-Straube, 1999). By mid-August, 10,047 (non-fungal) names had been registered (counting some duplications). In August, even though the trial run had functioned without hiccups for 19 months, the St. Louis Congress decided against registration, and exorcised all reference to it from the *Code* (Greuter et al., 2000). Registration offices (whose maintenance for two years had cost the IAPT ca. €60,000) were wound up by the end of the year.

2000 on: Registration had virtually become a non-word in botanical nomenclature. The main trace it left was maintenance of separate indexes of nomenclatural novelties in most of the accredited journals, to which they had agreed for becoming accredited. The registration database with its (eventually) 10,173 entries is still available for online searching (Greuter et al., 1999).

Registration at work

Registration was a low-cost operation. Design and implementation of the database, including screen frames for both data input and online query, cost IAPT a €2500 contract. The office was run by a half-time secretary for data input and a half-time taxonomic botanist screening the incoming literature, preparing data, and handling the correspondence with authors, publishers and registration centres. Input time for one name averaged six minutes, including optical scanning of the protologues for the purpose of archival documentation.

Input started by assigning each new name to its relevant category: new taxon or new combination or nom. nov.; fossil, algal, or other. Depending on the category, conditions for valid publication were queried and, unless confirmed, a 'caveat' or flag was generated (e.g. for a new fossil taxon: 'no Latin or English description or diagnosis'). For new combinations, the presence of a full and direct basionym

reference was checked, but not the basionym itself. Entered data included the name with standardised author citation, literature source (with stated date of publication), types (including specimen location) of names of new taxa, or basionym or replaced synonym of new names or combinations. The spelling was checked for correctable errors (this was my job), such as wrong terminations or connecting vowels.

An estimated 75% of the data came from accredited journals, the rest mostly from books and non-accredited journals. Only a tiny fraction were submitted by the authors themselves on the apposite (downloadable) forms, either directly or through a national registration centre. This proportion is bound to change if and when registration becomes mandatory.

Implementation in the *Code*

Fitting mandatory registration into the existing botanical *Code* (McNeill et al., 2006) requires remarkably little change. The proposals made to the Tokyo congress in 1993 were only four, and just two of them were essential:

- Addition of registration as a new, supplementary condition for valid publication of a name.
- Definition of the registration procedure as submission of the relevant printed matter to an accredited registration office. (Proposals to the St. Louis congress in 1999 would have modified this, permitting the submission of photocopies, under certain conditions.)
- Defining the date of a name as the date of reception at a Registration Office. This is an important point that has proved to be controversial, but is not central to the concept of Registration. Other solutions are possible.
- A clarification of what happens when someone wants to cheat by submitting not yet published names for Registration (they have to be registered anew once published) is of debatable usefulness and necessity.

Several important issues, as follows, were not explicitly mentioned, as it was felt that such details could be left for later clarification:

- Who is entitled to register a name? The implicit answer is anyone, if the author (or the publisher acting on his behalf), who are the first who can play the ball, fail to do so. Authorship is unaffected by this issue. It is assumed that, at least initially, the staff of the relevant registration centres will scan the literature for new, unregistered names and will register them on their own initiative.
- What status do registered names have? Initially it was thought by some that they would, by definition, be made valid and even perhaps legitimate by registration. This is not so. Whereas valid publication of the registered names of new taxa could, as a rule, be taken for granted, their legitimacy can not. It may, in particular, be affected by validly published, perhaps unindexed, earlier homonyms. As to new combinations, they may not even necessarily be new. Especially in groups or at ranks in which infraspecific names were not so far indexed, earlier validations of the same combination may exist.

Options for the future

Can (and should) anything be changed in a future registration system for botany with respect to what has been envisioned in the past? I can see at least one basic possible improvement, allowing for greater flexibility (and, hopefully, acceptability)

of registration. So far, registration has been seen as the last, supplementary step to be taken to achieve valid publication of a name fulfilling all other requirements. However, that chronology of events need not be mandatory. It is nowadays easily conceivable to have a name registered prior to its publication, in such a way that only the publication date must eventually be added. Authors would thus have a choice in structuring their taxonomic workflow.

It would be a minor step to turn registration into a cheap, quick, and easy alternative vector of validation. Authors could feed the data required for validation directly into an apposite online form, and these data would be promptly released in print (perhaps with a monthly or even weekly frequency) by the registration centre. This might even provide a source of income to the registration centre. Presumably, the original, rather cumbersome scheme of national Registration Centres can nowadays be dispensed with. Electronic communication has become safe, cheap and universally accessible, and while ordinary mail can and must not be ruled out as an available option, it will in practice all but disappear from the scene.

Registration will depend on the good will and full co-operation of existing indexing centres, as it did before. This can now be done without losing the unity of doctrine of the process as a whole. IPNI (International Plant Names Index) has demonstrated how well a multi-platform, shared-responsibility approach can function without the user even noticing it.

Should lists of registered names be published as hard copy? This has not so far been specified, although many may have assumed it tacitly. The option should be explored. Clearly, free online access to the information is paramount for today's user. Still, hard copy is unrivalled as a mean of safely archiving the data. Hard copy (which might include the new validations referred to above) could be generated quickly and cheaply directly from the computer, by means of an apposite formatting programme. Institutions not directly involved in the registration process may wish to support it financially by subscribing to the printed documents (or so I hope).

It would be a real boon if at least the elements required for valid publication, if not the whole protologues, could be made available on line. This will obviously depend on the agreement of copyright holders. In addition, automatic translation services (from English and perhaps other languages to Latin for authors, and conversely for users) should have become easily feasible by the time registration becomes functional.

In the medium term, once registration of names has proved its usefulness and has fully established itself in the mind of the biological community, the need will be felt to widen its scope to cover all nomenclaturally relevant acts, type designations in particular.

The way ahead

Registration is overdue. I could easily imagine that, once the present reluctance toward the registration concept is overcome by botanists, there will be a hue and cry for it to happen immediately, today better than tomorrow.

Undue haste can be detrimental, as can excessive restiveness. Should this meeting come to a positive conclusion on the desirability of registration, both in the botanical and zoological domain, then the upcoming IUBS General Assembly in Cape Town this year (2009) would, I believe, provide an ideal forum for deliberating and moving ahead. If this happens I might, hopefully, live to see registration become a reality.

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MycoBank, the virtual fungal laboratory of tomorrow

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MycoBank was officially launched by the CBS Fungal Biodiversity Centre towards the end of 2004, and announced in two papers published simultaneously in *Mycological Research* and *Studies in Mycology* (Crous et al., 2004a, b). It was envisaged as a freely available electronic depository system for taxonomic novelties in fungi (including lichens, oomycetes, other straminipiles, slime moulds, and yeasts). Since 2007, MycoBank has operated under the auspices of the International Mycological Association (IMA; www.IMA-mycology.org), and is directed by an international Scientific Advisory Board in regard to its terms of actions and policy. MycoBank presently contains three main elements:

A depository for nomenclatural novelties

This system allows mycologists to deposit new names, be it names of new taxa or new combinations. It also requires some basic information, both nomenclatural (for

example the basionym or type information) and taxonomical (e.g. a description). As soon as a name is deposited, the MycoBank system automatically e-mails a unique registration number to the depositor.

Before the deposit is accepted, the MycoBank software checks the uniqueness of the name: there is an automatic search for existing homonyms, and the depositor will immediately be warned if an earlier homonym exists. After the deposit, MycoBank administrators check the correctness of the name (for example for the correct termination, or typing errors) and will make suggestions when appropriate. MycoBank never applies any censorship, and a depositor may deposit anything he or she wants. The final judgement lies with the journal editors and reviewers, where it belongs. The correspondence between MycoBank and the depositor remains strictly confidential.

The deposited name is restricted until the name has actually been published; only in searches for homonyms may users be alerted to the existence of a name 'in press', but without any additional data, so neither the identity of the depositor nor the intended source of publication will be revealed. In some instances though (especially new combinations), potential authors have queried such names with MycoBank staff, who again contacted the primary depositor, leading to either cross references between two papers, or in some cases joint publications co-authored by all scientists involved.

The search for homonyms is based on the *Index Fungorum*. CBS is (with CAB International and Landcare New Zealand) one of the three custodians of *Index Fungorum*, and has contributed more than 100,000 new records, additions and corrections, including all new registrations (when published). At the moment the versions of the CAB International site and MycoBank are not completely identical, but we hope to have a working web-based system that automatically updates the respective hubs shortly.

The advantages of MycoBank over printed sources are obvious: (a) between 2004 and 2007, new names of fungi have been published in more than one hundred periodicals (excl. numerous books), and no indexing publication covers them all; (b) a time gap of up to 18 months between the publication of a new name and the listing in an index is not unusual; and (c) MycoBank allows the publication of additional data including descriptions, illustrations, DNA sequences, etc. Finally, MycoBank is a free service, and accessible from anywhere with internet access.

The acceptance of MycoBank by the mycological community is best illustrated by the following data: in 2006, 870 out of 1710 novelties were registered through MycoBank (approx. 50%). In 2007, 1445 novelties were registered, which is approx. 85% of the total for that year. Further, during 2007, 95 mycologists completed questionnaires at major mycological meetings in Baton Rouge (U.S.A.), St. Petersburg (Russia), and Léon (Spain) in August–September 2007 where 73 (85% of those voting) were in favour of registration of names in MycoBank being made compulsory for valid publication (Hawksworth, 2007).

In addition, the number of periodicals requiring MycoBank numbers as a prerequisite to acceptance for publication is increasing steadily. Judging from the sources of the total amount of newly published names in 2004, these periodicals already account for over 50% of the expected novelties, and these numbers are rapidly increasing. In fact, all leading mycological journals covering systematics in the ISI system (International Statistical Institute, Web of Knowledge) now require authors to deposit novelties in MycoBank, as does *Taxon*.

Additional taxonomic information

Depositors of nomenclatural novelties are asked to deposit, in addition to a description and type information, as much other data as they feel appropriate or have available. This includes illustrations, molecular sequences, physiological data, links to web-based data, etc. Also, data on existing taxa can be deposited, provided full information is given as to the source of the data, and a number of scientists are constantly updating MycoBank to complete the information for certain groups of fungi, or geographical regions. MycoBank at the moment contains over 25,000 descriptions and 8,000 illustrations, all with full references to either the original publication or, in case of unpublished material, to its owner. MycoBank now actively seeks collaboration with the managers of websites that contain authoritative information on fungi by providing cross-links directly to the information. These can be descriptions, illustrations, or literature.

The species bank concept

MycoBank contains additional taxonomic information, for example heterotypic synonymy, and an opinion as to the correct name. However, this can not be done by the present curatorial staff alone. The mycological literature is simply too vast to monitor and judge all developments, and this task can only adequately be accomplished by (groups of) dedicated specialists. At the moment, MycoBank contains eight species banks, which each contains a number of species, usually belonging to a taxonomic unit such as a family or several families (*Mycosphaerellaceae*, resupinate *Russulales*), but sometimes one or several genera (*Aspergillus*-*Penicillium*), an ecological group (medical fungi), or morphological unit (yeasts).

A species bank contains, besides rather complete sets of descriptions and illustrations and heterotypic synonymy, structured morphological and molecular data, allowing, for example, polyphasic identification. In the near future it will be possible to curate species banks on-line, and the IMA will actively stimulate individual researchers or research groups to adopt a larger fungal taxon in MycoBank. These specialists or groups of specialists will have full rights and control, and will have the final responsibility for taxonomic decisions. Simultaneously, a Wiki-type system is also to be provided, so that different taxonomic opinions can also be viewed.

Prospects

The increasing acceptance of MycoBank by mycologists is a reason for confidence that the International Code of Botanical Nomenclature will make the deposit of new names of fungi in MycoBank a condition for valid publication at the next International Botanical Congress in Melbourne in 2011, subject to approval at the International Mycological Congress which will debate the issue in Edinburgh in 2010. As a consequence the mycological community has the prospect of names *plus* a minimum set of data available freely via the internet immediately once they are published.

It is also feasible that, when the *Index Fungorum* has reached a very good coverage of the available names, the nomenclatural past will become a closed system by giving only those names the status of availability. That would end the current situation where the literature is a black box containing numerous previously unnoticed names or ones of uncertain application that are a threat to well-established names and a

constant source of instability. This would allow a situation that the bacteriologists faced in 1980, but without the drawback of facing numerous taxa without an available name.

Finally it is hoped that the possibility to adopt groups of fungi will be attractive to scientists, and that this will enhance MycoBank as a much-used reference source. New developments under consideration, such as linking species banks to strains, and DNA Barcodes, will undoubtedly add further to the usability of the system.

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Registration of names: the bacteriological experience

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Prokaryote nomenclature is governed by the International Code of Nomenclature of Bacteria (Lapage et al., 1992), which like all *Codes* of nomenclature regulates nomenclature, requires the formal description of named taxa, and uses the principle of types. There are a number of similarities with the botanical *Code* (from which it was derived), but there are a number of significant and revolutionary differences that make the bacteriological *Code* one of the pioneering works that other *Codes* of nomenclature have yet to emulate or even appreciate in full.

Like all nomenclatural systems dealing with the names of organisms, the bacteriological *Code* has had to deal with an increasing number of names of dubious value and a constant source of confusion (Skerman et al., 1980; Lapage et al., 1992; Sneath, 2005). However, during the 1960s a small group of bacteriologists laid the cornerstone of a system that has revolutionised prokaryote nomenclature and put it in the enviable position of being able to trace all names in use under the *Code*, as well as indicating which names are considered to be synonyms (Lapage et al., 1992; Sneath, 2005). The present system relies on two key features that were introduced during the 1970s.

Firstly, it was decided to collect all known names of prokaryotes, and to evaluate them according to the existence of appropriate descriptions and adequate typification (Skerman et al., 1980; Lapage et al., 1992; Sneath, 2005). The goal was to draw up an accurate list of names that were to become the basis of a list of protected names (*Approved Lists of Bacterial Names*; Skerman et al., 1980), and to discard all names that did not fit the essential criteria of a modern code of nomenclature, i.e. an

adequate description and typification. All names that did not conform to these criteria lost standing in nomenclature and, unlike the names on the *Approved Lists of Bacterial Names*, were not considered to be 'validly published'. These names served as the starting point for the new nomenclature (1 January 1980), although it is important to emphasise that all names on that list make reference to the original authors and the dates of publication of those names. During the course of the compilation of the *Approved Lists of Bacterial Names*, some 30,000 names were examined, and less than 2,500 names at the rank of class to subspecies were retained.

Secondly, there seemed little point in creating the list of protected names if the *Code* were not to provide a mechanism by which the problems of the past were not to repeat themselves. The solution that was formulated consisted of checking that names conformed with the requirements of the bacteriological *Code*, i.e. are accompanied by a description and a type designation. Although the bacteriological *Code* refers to this process as 'valid publication of a name' it is significantly different from the equivalent term in the botanical *Code*. In order for a name to be validly published under the bacteriological *Code* it must conform to the rules of the *Code*, a formal act of registering *Code* compliant names. Contrary, to popular misinterpretations this does not include any form of censorship (Tindall et al., 2006).

Thus, modern prokaryote nomenclature is based on a list of protected names (that cannot be replaced by earlier names), serving to secure a link with the past, and a continually growing list of new names that are officially registered, i.e. confirmed as being compliant with the *Code*. This system is unique, has operated since 1 January 1980, and has created a comprehensive, valuable system that serves as a goal that botany and zoology have yet to reach.

The bacteriological *Code* has also adopted a traditional approach to the way names are registered (i.e. validly published), in the form of peer reviewed journal or monograph publication, with notification of registration being made *via* a single organ, the *International Journal of Systematic Bacteriology* (now the *International Journal of Systematic and Evolutionary Microbiology*), itself being in the hands of the International Committee on Systematic Bacteriology (now the International Committee on Systematics of Prokaryotes). However, that traditional approach does not meet the requirements of a modern global community of bacteriologists. Relying on the printed word is no longer adequate, and increasing use is being made of the advantages that internet communication has to offer. In particular, the existence of expertly curated lists of names, with accurate reference to the date and authors of valid publication of the name, indication of the location of the description (and the appropriate experimental work), the location of the material that typifies species and subspecies, as well as indicating synonymies, provides a widely distributed global network of end users with rapid access to the critical elements of the taxonomic literature. Having such information available is equally valuable to those describing new taxa, those identifying existing taxa, and those who may only be interested in the nomenclature for other reasons, such as regulatory authorities. The most comprehensive list is that available at the 'Lists of Prokaryote Names with Standing in Nomenclature (LPSN)' (www.bacterio.cict.fr), and others that can be recommended are the 'Taxonomic Outline of the Bacteria and Archaea (TOBA)' (www.taxonomicoutline.org), and 'Bacterial Nomenclature up-to-date' www.dsmz.de/microorganisms/bacterial_nomenclature.php.

The *International Journal of Systematic and Evolutionary Microbiology* (IJSEM), published by the Society for General Microbiology, also provides an open access service to the original literature published in the pages of the journal (ijs.sgmjournals.org). This policy makes all publications available free of charge two years after the date of publication. The journal also participates in the WHO HINARI and UN (FAO) AGORA (access to scientific literature) programmes, allowing access without charge, to eligible institutions in World Bank List 1 countries, immediately on posting on the internet. All back issues of the *International Journal of Systematic Bacteriology* and the *International Bulletin of Bacteriological Nomenclature and Taxonomy* (predecessors of the IJSEM) are now available online without charge. This gives access to all institutions now interested in these publications that have never subscribed to the original printed versions.

The advantages of the current system should be obvious, providing access to even the casual users who would otherwise not have access. The availability of complete and accurate lists of names is of immeasurable importance, although probably not fully appreciated. To date, only virologists have come anywhere near to achieving a similar goal. The value of both the registration of names and the availability of those names in a modern form of communication puts the bacteriological *Code* at the forefront among the traditional *Codes* of nomenclature.

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The Cultivated Plant experience

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Uniquely among organisms, cultivated plants are covered by two *Codes* of nomenclature: the International Code of Botanical Nomenclature (ICBN) which deals with the genus, species, infraspecific ranks, and hybrids; and the International Code of Nomenclature for Cultivated Plants (ICNCP) which is applied to plants selected or used by people, and uses the categories of grex, group, and cultivar. The concept of registration for cultivated plants actually predates the first ICNCP, which was drawn up in 1952, and grew out of the need to record the names of new cultivars which are often only to be found in ephemeral literature such as nursery catalogues,

seed lists and gardening magazines. Frequently these publications are imprecise in date or even undated, so there was a need to establish a system for determining priority. This was realised in a system of registration which was first proposed for cultivated plants during the International Horticultural Congress in 1930 which recommended the drawing up of lists of names for particular groups of plants. The first such lists included those for irises, tulips, delphinium, orchids, and daffodils. Many of the bodies that produced these lists are today still the official registration authorities. The first classified list of daffodil names was published by the RHS in 1908 and was 31 pages long, while in 1998 the Register was 1166 pages long and contained the names of over 26,000 cultivars.

The present system of horticultural registration operates under the International Commission for Nomenclature and Cultivar Registration, which is a body constituted by the International Society for Horticultural Science (ISHS). The Commission is responsible for appointing and monitoring the International Cultivar Registration Authorities (ICRAs) of which there are currently over 70 worldwide and which are listed on the ISHS website (www.ishs.org/sci/icra.htm). Registration is carried out by individuals (Registrars) on behalf of the ICRA, who receive applications to register new names from growers, hybridizers and nurserymen all over the world. They also scan literature (both recent and old) to build up a comprehensive listing of names in the group for which they are responsible. Such a group may be a single genus, species, or similar, but not closely related plants (such as bulbs) or a geographically defined group.

Under the ICNCP, registration is defined as 'The act of recording a new name or epithet with a registration authority.' It is not a requirement for valid publication and is not mandatory for establishment of a new name. This has to be the case as the combined coverage by ICRAs is not universal and there are many plant groups for which there is no registrar. While the concept of priority is fundamental, a registrar has the authority to set this aside in certain circumstances and can even allow the re-use of a cultivar epithet for a different plant. The registrar is required to publish a full register from time to time and this can itself be a means of validating a name. In registering a new name, the registrar must check whether a proposed name has been used before and whether it meets the requirements of the current ICNCP. If the application fails, then the registrant is invited to re-submit the registration once the reasons for its failure have been addressed. Successful applications are generally recognised by a certificate as proof of registration.

Although the system has been in place for over 50 years, the process does not enjoy the status and rigour of the scientific *Codes* of nomenclature. As pointed out earlier, it is entirely voluntary and there are no nomenclatural consequences for not registering a name; the system is not universal as there are horticulturally important groups that lack an ICRA and it is dependent upon the support of organisations to be willing to undertake registration activities. That said, some invaluable work has been carried out to document and make available comprehensive lists by some ICRAs – most notably but not exclusively for camellias, heathers, irises, syringas, and geraniums, as well as those produced by the RHS. It has been encouraging to see ICRAs starting up around the world, such as the Chinese Flower Association (Sweet Osmanthus Branch) and the American Brugmansia and Datura Society.

The cultivated plant *Code* is much more affected by commercial concerns than other *Codes*, which means both statutory and legal issues can prevail over purely

nomenclatural considerations. Its user group is diverse and not generally attuned to the niceties of rules for the correct naming of plants, especially when they get in the way of making a living. A registrar therefore has to be flexible and as helpful as possible when dealing with plant naming problems.

In registration, the commercial requirements can conflict with nomenclatural rules in relation to Plant Breeders' Rights (PBRs) and selling names, or 'Trade Designations' to give them their more formal term. PBRs provide a system for protecting new cultivated varieties of plants by an intellectual property right which is established by an International Convention and operates through an international body (UPOV). There are national statutory plant registration authorities that register PBRs, and once granted a PBR is legally enforceable. As a consequence the cultivated plant *Code* has ruled that a PBR name takes priority over any existing name. Since a statutory plant registration authority is not required to check whether a proposed name has already been registered with an ICRA, registrars must make themselves aware of any proposed PBRs. However, a PBR is geographically limited and is not permanent so can lapse if the registrant decides not to renew the application, although it will still retain its priority under the ICNCP.

'Selling names' arise when names are needed to sell the plants and the cultivar name is not thought appropriate. These can vary from country to country, and a 'selling name' may be a translation or transliteration of the correct cultivar name into the language of the country where it is being sold. 'Selling names' are not governed by the ICNCP, although a registrar does need to be aware of the application of selling names when considering the registration of cultivars. The use of trademarks to protect plant names is another complication as the rules for these vary according to national legislation and are not always well understood by nurserymen.

For horticulture, while registration by ICRA has provided the basis for stabilisation of nomenclature for the reasons explored above, it still has some way to go before it can be said to fully address the needs of all its users. Looking ahead, future developments should include greater harmonisation between ICRA registration and UPOV (PBRs); encouragement of registration by making it easier to effect (such as registration online) and to devise a system for logging cultivar names for groups not covered by an ICRA. All these opportunities are being explored and, building on the solid work of the registrars to date, it will be possible to bring about a more comprehensive and widely supported system of registration for cultivated plants.

The vision for zoology: ZooBank

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ZooBank was originally conceived as an open-access register for all scientific names of animals, new and retrospective, back to 1758, and formally proposed by 29 internationally acclaimed zoologists in 2005 (Polaszek et al., 2005). The first version of ZooBank, with 1.6 million animal names, was provided by Zoological Record

(Thomson Zoological Ltd) in August 2006, and provided a popular online service until late 2007. At that point in its development, discussions between ICZN (International Commission on Zoological Nomenclature, the initiators of ZooBank), Thomson Zoological, the Global Biodiversity Information Facility (GBIF), and the Taxonomic Databases Working Group (TDWG; currently Biodiversity Information Standards) resulted in the transfer of responsibility for the development of ZooBank to Commissioner Richard Pyle at the Bishop Museum, Hawaii, where it is currently based.

Registration of animal names and nomenclatural acts with ZooBank will undoubtedly increase the rate and ease of description of the planet's animal biodiversity, by facilitating access to novel and retrospective zootaxonomic data. Adopting the GenBank model, where publishers of scientific papers require authors to provide universally accessible data (molecular in the case of GenBank, nomenclatural and taxonomic in the case of ZooBank), will eventually result in a mandatory registration system for the scientific names of animals, ensuring the universal visibility of the scientific names of animals and all nomenclatural acts in zoology. The two crucial differences between GenBank and ZooBank are: (a) the eventual mandatory requirement for registration with ZooBank under the International Code of Zoological Nomenclature; and (b) that the primary stakeholders in ZooBank are expert zootaxonomists.

Why is ZooBank vital for the future of animal taxonomy, and hence all animal biodiversity studies? A primary requirement for facilitating biodiversity studies is to increase the visibility of taxonomic and nomenclatural acts. Many of these are effectively 'hidden' in thousands of journals and other publications, many extremely difficult to obtain. With ZooBank, taxonomic data will not only be freely available, but an alerting service using RSS feeds will also be provided for those taxa of interest to the user.

The idea of a mandatory registration system implemented *via* ICZN may, for some, have authoritarian overtones. However, the opposite is actually true. The universal visibility of and access to all animal names and nomenclatural acts will effectively open up, democratise and make available the fundamental and inclusive support system for biodiversity studies – animal taxonomy. In order to achieve completeness of the ZooBank resource, integrating registration of new names with retrospective registration of all animal names back to 1758, the mandatory aspect is crucial. In this way, ZooBank will become a primary source of authoritative animal names and data. To give a medically important example, the primary causative organism of the disease giardiasis is the protozoan *Giardia lamblia*. The primary database of organism names, *Zoological Record*, contains seven variant spellings of the species name: *lablia*, *lambia*, *lambila*, *lambla*, *lamblia*, *lamblia*, and *lamlia*, without any indication of which is correct. This example makes very obvious the need for a central, authoritative source of taxonomic information.

The establishment of ZooBank provides an opportunity to introduce unprecedented stability into zoological nomenclature by, among other things: automatic checking for code-compliance; prevention of homonymy; stabilisation of spelling; fixing of genders and stems; stability in gender agreement; and quality control.

The current *Code* permits certain descriptions or other taxonomic acts that actually run against its main principles, for example the description of new taxa in the absence of type specimens runs directly counter to the Principle of Typification. This provision is of course essential for endangered or otherwise illegal, unethical, or impossible to

collect species, but in its present hazy form requires some simple additional legislation by the community acting through the Commission to prevent future problems and possible abuses. There is a growing concern – to some extent connected to the previous provision – about the auctioning or otherwise selling of ‘naming rights’, for which some intervention by the *Code* is also required. However, the most important change needed to the present zoological *Code* concerns publication criteria. Currently, the *Code* does not permit electronic-only publication, and this is, and will increasingly become, a problem, as journals quite understandably reduce their hard-copy output and move towards online-only publication. While online only taxonomy may be highly desirable, and the logical next step, without a mandatory registration system that both facilitates and regulates the process chaos will result.

Registration in ZooBank is envisaged as a relatively straightforward task, with an online proforma requiring all the data available for code-compliance and prevention of a large proportion of future nomenclatural problems requiring Commission intervention. Both primary and third party registration will have to be permitted, and there will be important differences between pre- and post-publication registration, with a holding period in the pre-publication scenario where the as-yet unpublished names are protected. Built-in spell-checks will detect homonyms and enable the correction of gender agreement, while drop-downs will greatly facilitate the process, for example with very frequently-cited historical literature. Links to exact copies of original descriptions and figures, and no limit to the amount of data (e.g. figures, gene sequences) associated with new descriptions will be made via Morphbank, GenBank and other universally accessible free resources. This will lead in many cases to inevitable problems over authors’ and publishers’ copyright, heralding a radical but essential change in attitude towards copyright in animal systematics and eventually further afield. Above all, in order to succeed, registration with ZooBank must be straightforward, it must be free, and the resulting data must be freely, universally accessible.

There are still many unknown factors in the development of ZooBank. The last experience in botany was ultimately unsuccessful, but perhaps because it was premature. To date (2007) we have had 2½ years of public discussion and debate, especially facilitated by e-mail discussion lists, wikis, and emerging projects such as CATE, and EDIT. GBIF and TDWG are currently taking an active role in these activities. Through animal taxonomists working together openly we can show that mandatory name registration is neither authoritarian nor imperialistic, but is actually both authoritative and democratic. Clearly additional resources will be required, and some high-profile groups need to be showcased successfully in the very near future, but the spirit of cooperation that has led to ICZN *Code* adherence being one of the best examples of international scientific cooperation will ensure the success of ZooBank.

Reference

Polaszek, A. et al. 2005. A universal register for animal names. *Nature*, **437**: 477.

The debate

The Chairman for the evening session, Richard Fortey (Department of Palaeontology, Natural History Museum, London), reminded all of the content of the earlier presentations and explained that he would begin by asking for a preliminary vote on

the motion ‘The registration of new names of organisms should be compulsory’, and that there would be a final vote after the debate.

The Proposer, John McNeill (Royal Botanic Garden, Edinburgh), reviewed the afternoon’s presentations and the different ways in which registration of new names was being dealt with by the different *Codes*. He stressed that the present proposal dealt only with new names, and would not replace existing systems but support them and the accompanying indexing systems. He considered the financial implications of possible cuts to funding for maintaining indexes if registration of names is not seen as mandatory, and went on to discuss some of the obstacles met by earlier schemes before the current global network was in place providing new solutions to these. He concluded by stressing that any registration system should be: (a) easy; (b) accessible; and (c) community based.

The Opposer, Alain Dubois (Muséum National d’Histoire Naturelle, Paris), considered the taxonomic impediment of naming all new organisms, the geographical distribution of papers on new taxa and their authors, stressing the differences between the contributions from the countries of the ‘North’ and those of the ‘South’, together with their differing levels of information technology (IT) access, the threats to IT, and available access to the older literature supporting taxonomy. All these create major problems for those working in such regions. He also drew attention to the present unbalance in ZooBank geographical representation. He considered that the key solutions to stabilising names were the monographic publications featuring major revisions, but these would often be traditional paper publications with low impact factors. The support provided for them by subscriptions, and exchanges within the academic community, was essential if they were to survive in the face of growing competition from consortium or package sales for access to commercial online journals. He feared that mandatory registration, assuming it could be implemented and regulated, could lead to the loss and abandoning of paper copies, divisions among the biological community, and waste of energy and time in activities such as those proposed by supporters of the PhyloCode. The 250th anniversary of the 10th edition of *Systema Naturae* should not be used as an excuse to impose new systems, but instead ICZN should just add ZooBank as an additional tier to the existing nomenclature procedures and see what happens over time.

Contributions from the floor were made by speakers from the afternoon session, together with George Garrity, Sandra Knapp, Rafael Govaerts, Suzanne Walker and others. Votes cast before and after the debate were as follows:

	Yes	No	Don’t know	Total voting
Before	42	19	2	63
After	38	24	1	63

The motion was therefore passed, but not by an overwhelming majority of those present.

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Proposed corrections to the entries on the Official Lists and Indexes for five family-group names, five genus-group names and one species-group name (Mollusca)

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Abstract. The purpose of this proposal is to correct entries to authorship, date of publication and, in some cases, status of family, genus and species-group names already placed on their respective Official Lists or Indexes.

Keywords. Nomenclature; taxonomy; Gastropoda; Bivalvia; ACERIDAE; AKERIDAE; BITHYNIIDAE; HYDROBIIDAE; MARGARITIFERIDAE; *Acera*; *Akera*; *Balcis*; *Eucampe*; *Margaritifera*; *Ocenebra*; *Eucampe donovani*; *Akera bullata*; gastropods; bivalves.

Several entries of Molluscan names made on the Official Lists and Indexes were found to contain errors, which are here proposed to be corrected. None of the proposed corrections results in the change of the valid name for a taxon, but will honour the published data and the Code. The intended effects of the relevant Opinions are not negated.

A discussion of nomenclatural aspects of three works precedes the listing of proposed corrections, in order to avoid identical comments under several of the names here treated.

(1) Leach, W.E. (with contributions by Gray, J.E.). Oct. 1847. The classification of the British Mollusca. *Annals and Magazine of Natural History*, (1) 20 (133): 267–273. [published posthumously by Gray, J.E.]

Four Opinions treated names proposed in this work, but attributed authorship inconsistently, viz. to ‘Gray, 1847’ in Opinions 539 and 886; to ‘Leach in Gray, 1847’ in Opinion 1739; to ‘Leach, 1847’ in Opinion 1942. It is here proposed to attribute all names made available in the work in question to Leach, 1847. The divergence of opinion came about by the fact that this work was published posthumously by Gray, who edited it and for which he wrote a preface. The history of Leach’s posthumous works is described by Gray (in Leach, 1847 and 1852), and Gianuzzi-Savelli & Gentry, 1990.

The posthumous works of Leach were: (1) ‘The classification of the British Mollusca’, a checklist of names without any descriptions, which was completed in 1818; Gray in Leach (1847) mentions also an 1816 version; (2) The ‘Synopsis of the Mollusca of Great Britain’, containing descriptions and figures of many species, was partly printed in 1819 or 1820 and proofsheets distributed to several interested naturalists. This act does not constitute publication for nomenclatural purposes (Article 9.3 of the Code), but resulted in Leach’s unpublished names becoming widely known. Likewise, copies of the ‘Classification’ were available to several naturalists. Furthermore, Leach’s collection was available to Gray (Gray in Turton, 1840, p. 1)

and others. Gray (in Leach, 1847, p. 268) believed that Leach's names were already available from 1818 due to Leach distributing his 'Classification' to others. Nonetheless Gray undertook to publish Leach's checklist, probably out of the realisation that it had not yet been properly published. Gray added an available species-group name to some of Leach's names and thereby involuntarily helped these become available, insofar as they had not been made available earlier by other authors. Although it may seem that Gray was responsible for the availability of some of Leach's names, and is therefore their author, he actually acted only as an editor: the information required to identify Leach's names originated necessarily from Leach's manuscripts and collection. Gray refrained from statements on the validity of Leach's names, but Gray's own near-simultaneously published paper (Gray, 1847) shows that he considered some of them as junior synonyms. Amongst the four genus-group names discussed here, three are so treated: *Haminaea*, *Eucampe* and *Ocenebra* (Gray, 1847, nos. 307, 303, 10), as is the species-group name *Eucampe donovani*. It is thus evident that he treated the Leach's 1847 paper as a representation of Leach's work alone, without any intention to express his own views on the validity of Leach's names.

(2) Schumacher, C.F. 1815 ('1816'). Afhandling over conchyliologiske Systemer, og om nogle toskallende Conchylier. Oversigt over det Kongelige Danske Videnskabernes Selskabs Forhandlinger og dets Medlemmers Arbejder i de sidst to Aar, [1813–1815]: 7.

The publication date of Schumacher's work is usually given as 1816, but according to Gosch (1878, p. 217) it was already published in 1815. Gosch did not cite specific evidence for the 1815 publication date, but his 'Overview of Danish zoological literature' is a credibly detailed and comprehensive account. Inspection of the periodical resulted in several observations consistent with an early publication date. It is proposed to accept the 1815 date on the basis of Gosch's statement and the following indications:

(1) Schumacher's brief article was published on p. 7 in an 'Overview of the transactions of the Royal Danish Science Society, and of the works of its members in the last two years, by Prof. Ørsted, Knight of the Dannebrog, Secretary of the Society'. The volume does not bear a number, and a publication date of 1816 is only given on its last page (p. 42). The next issue of the same periodical reports on the works of the members of the Society from 1 November 1815 to 31 May 1816, and bears also a publication date of 1816 on the last page (p. 30). This confirms Gosch's statement that the 'last two years' in the title of the first issue refer to 1813–1815, or more precisely to a 2 year period ending before 1 November 1815. The reporting periods in subsequent issues of this periodical usually ended on 31 May, so this may hold true also for the 1813–1815 issue. Publication within the remainder of 1815 would then be well within the reach of contemporaneous printing capabilities.

(2) The latest date mentioned in the texts is 1814 (pp. 5, 6, 9, 30).

(3) The 1815–1816 issue contains 30 pages, while the 1813–1815 issue contains 42 pages, of which the content of the first 30 pages only corresponds to the title, the remainder being an announcement of prizes for the solution of certain scientific questions, for which submissions had to be made by December 1816. Such announcements should have been published well in advance of the deadline, thus publication in 1815 seems to be a reasonable assumption. It is also conceivable that

pp. 31–42 (which alone bear the printed date 1816) were published later than pp. 1–30 (i.e. the latter in 1815, and the former in early 1816), or that the date of 1816 was printed as the anticipated publication date, be it for the entire volume or only for pp. 31–42, whereas publication was already achieved in 1815.

To avoid confusion it may be mentioned that Schumacher's article was reprinted almost literally on p. vi of an overview of the works of the Society's members from '1814 to 1822' (but the volume actually refers to the period ending 31 May 1824 (p. xxxvi)).

(3) Troschel, F.H. 1856–1863. *Das Gebiss der Schnecken zur Begründung einer natürlichen Classification untersucht*, 1. Livr. 1: 1–72, pls. 1–4 (1856); livr. 2: 73–112, pls. 5–8 (1857); livr. 3: 113–152, pls. 9–12 (1858); livr. 4: 153–196, pls. 13–16 (1861); livr. 5: i–viii, 197–252, pls. 17–20 (1863). Nicolai, Berlin.

Troschel, throughout his work (1856–1863), used the endings –IDAE or –ACEA for family group names, the rank of which he stated as such. The rank of the name 'HYDROBIAE' and of several other suprageneric units with similarly formed names on pp. 95–129 is not given. The reason for this different treatment is explained on pp. 94–95 (see Bouchet et al. 2005 (p. 6) for a translation from German and further discussion): Troschel was unsure about the familial classification of certain genera, and therefore decided to 'discuss the genera in small groups, without wishing to assign to them the value [= status] of families.' Therefore, the names so formed are treated as temporary references (Article 1.3.5 of the Code) which are unavailable for nomenclatural purposes. Alternatively, these group names could perhaps be interpreted as of subfamily or tribe rank, which would then have been proposed without a family assignment. This interpretation is not followed here, as Troschel did not recognise tribes or subfamilies; he merely grouped genera with similar radula characters, without coming to a conclusion on the taxonomic significance of these characters.

Family-group names

The following five family-group names are cited in the *Official Index of the Rejected and Invalid Family-Group Names in Zoology*:

ACERIDAE Odhner, 1907 (p. 350)

The name was placed on the *Official Index of Rejected and Invalid Family-Group Names in Zoology* (Opinion 539, Opinions and Declarations 20: 65–76, February 1959).

In Opinion 539, the name was rejected as an Erroneous Subsequent Spelling of AKERIDAE Pilsbry, 1893.

Proposed correction of authorship: de Kay, 1843, p. 14.

Proposed correction of the statement: The statement should be changed from 'Erroneous Subsequent Spelling' to an 'Unjustified Emendation' of AKERIDAE Sander-Rang, 1829 by de Kay, 1843, p. 14. Prior to Odhner, 1907, the spelling ACERIDAE had also been used by Mazzarelli, 1891 (p. 243).

AKERIDAE Pilsbry, 1893 (p. 350)

The name was placed on the *Official List of Family-Group Names in Zoology* (Opinion 539, Opinions and Declarations 20: 65–76, February 1959).

Proposed correction of authorship: AKERIDAE Sander-Rang, 1829 (p. 146).

The actual publication date of Pilsbry's work is 1895. Bouchet & Rocroi, 2001 (pp. 171–172) proposed to change the authorship in the Official List entry to Mazzarelli, 1891 (p. 243, as family ACERIDAE). However, in the period 1822 to 1847 numerous uses of a family name based on *Akera* Müller, 1776 or its unjustified emendation *Acera* Cuvier, 1810 exist. Bouchet & Rocroi (2001, 2005) regard these as descriptive terms not based on an available generic name, but the relevant publications show clearly that it is derived from the generic name *Akera* Müller, 1776. Analysis of the nomenclatural status is made difficult by incorrect statements and, in terms of the current Code, illegal actions of several authors, as will be discussed below.

Sander-Rang (1829) proposed the family name as '2e. famille. Les Acères, Cuv. Bulléens, Lam. [Lamarck]. Acérés, Lat. [Latreille].' Included genera are 'Acère, *akera* Cuv.', 'Bulle, *bulla* Lin.', *Gasteropteron* Meckel, 1809 and '*Sormetus* Adanson' [= *Somertus* Férussac, 1822]. The genus *Bulla* Linnaeus, 1758 is divided into the subgenera 'Bulles proprement dites' and 'Bullées' [*Bullaea* Lamarck, 1801]. Sander-Rang apparently did not accept the family name Les Bulléens Lamarck (1819) (actually attributable to Rafinesque, 1815, see discussion by Bouchet & Rocroi, 2005 (p. 40)) because he subordinated the type genus *Bullaea* Lamarck, 1801 as a subgenus of *Bulla* Linnaeus, 1758. *Bullaea* is a junior subjective synonym of *Philine* Ascanius, 1772. It is not confamilial with *Akera* Müller, 1776, but belongs to the family PHILINIDAE Gray, 1850 (1815). Sander-Rang (1829) and most contemporaneous authors attributed the family-group name incorrectly to Cuvier, but Cuvier (1810, 1817) treated 'les Acères' as a genus and, moreover, attributed the authorship of this genus to Müller, 1776. For further details see the discussion below under the genus-group name *Acera*. The first author to give family rank to 'les Acères' is Férussac, 1822 (p. 30), who introduced the name as '2e Famille. Les Acères, Cuv. *Akera*, Muller.' The quotation of a genus-group name as synonym of a family group name is incorrect, but it demonstrates that Férussac's family name is indeed based on *Akera* Müller and is not an invented descriptive term. The family name is not available from this publication, because Férussac did not treat *Akera* Müller, 1776 as a valid genus group name: he listed its type species, *A. bullata* Müller, 1776, in this family in the genus *Bulla* Linnaeus, 1758 (incorrectly cited as '*Bulla* Lamarck') under the name of its objective synonym *Bulla akera* Gmelin, 1791; *Akera* s. str. sensu Cuvier is cited as a synonym of *Doridium* Meckel, 1809. Subsequent authors before Sander-Rang, 1829, did not make the family-group name available for the same reason: Latreille, 1824 (table, as Acères); Latreille, 1825 (p. 177, as Acérés); Blainville, 1825 (p. 476, as Acères, *Akera*); Risso, 1826 (p. 48, as 'les Acères Cuv. '); Menke, 1828 (p. 6, as *Acerae*). Sander-Rang, 1829, attributed the genus name *Akera* incorrectly to Cuvier, despite the latter's (1810 (as *acera*), 1817 (as *Akera*)) unequivocal reference to Müller, 1776. Sander-Rang's statement: 'Une seule espèce, l'*A. carnosa*, de la Méditerranée a servi à l'établissement de ce genre dont M. Cuvier a fait connaître l'organisation . . .' is incorrect, as Cuvier's reference (1810, 1817) to Müller shows he did not 'establish' the genus. Instead, he defined a subgenus 'Acères proprement dites' (1810) with '*Bulla* *carnosa* Cuvier, 1810 as only included species; in the genus '*Acera*', Cuvier (1810, 1817) included as other subgenera *Bulla* Linnaeus (sensu Lamarck) and *Bullaea* Lamarck with several named species. Cuvier's restriction of *Akera* s. str. to

the group exemplified by '*Bulla*' *carnosa* Cuvier 1810 [= *Aglaja depicta* Renier, [1807]] pays no regard to the type species of *Akera* Müller, which falls into the genus or subgenus *Bulla* as understood at the time. The valid name for the taxon named *Akera* s. str. by Cuvier, Sander-Rang and their contemporaries is *Aglaja* Renier, 1807, of which *Doridium* Meckel, 1809 is a subjective synonym. This was already recognised by Cuvier, 1817 who cited *Doridium* Meckel, 1809 in synonymy of *Akera* s. str. Opinion 1079 (BZN 34: 16–20, July 1977) confirmed the valid name for this taxon as *Aglaja* Renier, [1807].

The fact that the family-group name AKERIDAE Sander-Rang, 1829 is based on a misidentified type genus is no obstacle to using it in the taxonomic sense determined by the correctly interpreted type genus. Article 65.2.1 stipulates that such cases should be referred to the Commission if stability or universality is threatened, or confusion is likely to be caused. This is not the case here; consequently the Commission is merely asked to correct the authorship and date of the family group name AKERIDAE.

Sander-Rang, 1829 used only a vernacular term, but Article 11.7.2 concerning family-group names originally introduced as vernacular names is considered as satisfied, as the name became accepted by most authors of the time who discussed the taxa in question, and was latinised several times (see below). It should be noted that Article 11.7.2 requires such names, inter alia, to be generally accepted as dating from their first publication in vernacular form. The latter condition is here regarded as implicitly fulfilled, because Sander-Rang (1829) as well as subsequent authors attributed the name to Cuvier. Although this is incorrect, it shows that all authors referred to the same taxon, viz. *Akera* sensu Cuvier. Note that the word 'Acera' or 'Akera' is a word in a Greek plural form (Article 11.7.1.1 of the Code), if this spelling was used as a family-group name, but as a genus-group name it is a word in the singular, treated as a Latin feminine noun despite its derivation from the Greek.

Between 1829 and 1847, the family name AKERIDAE was used, for example, by Deshayes, 1830 (p. 553, as ACERA); Menke, 1830 (p. 12, as ACERA (Acères, Cuv.)); Potiez & Michaud, 1838 (p. 57, as les Acères Müll. Cuv.); de Kay, 1843 (p. 14, as ACERIDAE), Nyst, 1845 (p. 450, as 'les Acères Cuv.'), Herrmannsen, 1846 (pp. 8–9, as ACERA Férussac and ACERAE Menke), and Gray et al. 1846 (p. i, as ACERA). Thereafter, the name AKERIDAE and its spelling variants fell into disuse, possibly because of Gray's (1847) actions: he placed *Akera* Müller into BULLIDAE: BULLINAE, and reinstated *Doridium* Meckel, 1809 with the synonym '*Acera* Cuvier 1817, Lamarck 1822' [meaning *Akera* Müller sensu Cuvier 1817 and sensu Lamarck 1822] in BULLIDAE: DORIDIINAE. Not before the anatomical studies of Mazzarelli (1891) was the genus *Akera* separated from the family BULLIDAE. The nomenclatural status of *Doridium* Meckel, 1809 and DORIDIINAE Gray 1847 was settled in Opinion 1079 (1977), whereby these two names were suppressed in favour of *Aglaja* Renier, [1807] and AGLAJIDAE Pilsbry, 1895.

BITHYNIIDAE Troschel, 1857 (p. 101)

The name was placed on the *Official List of Family-Group Names in Zoology* (Opinion 1664, BZN 49: 78–80, March, 1992):

Proposed correction of authorship: Gray in Turton, 1857 (p. 24).

The authorship of the name BITHYNIIDAE was originally recorded (Opinion 475, *Opinions and Declarations* 16: 307–330, July 1957 (p. 312)) as of Gray, 1857, but then changed to Troschel, 1857, for priority reasons. Troschel's name 'Bythiniae' was intended as a temporary reference which is not available for nomenclatural purposes (Article 1.3.5 of the Code); see discussion by Bouchet et al. 2005 (pp. 6, 37), and above. BITHINIADAE Gray in Turton, 1857 is an incorrect original spelling which was placed on the Index in Opinion 475.

HYDROBIIDAE Troschel, 1857 (p. 106)

The name was placed on the *Official List of Family-Group Names in Zoology* (Opinion 2034, BZN 60: 152–154, July 2003).

Proposed correction of authorship: Stimpson, 1865 [a] (p. 52).

See discussion of Troschel's names by Bouchet et al. 2005 (pp. 6, 37), and above. The name HYDROBIINAE was made available by Stimpson, 25 Feb 1865 [a] (p. 52) under Article 12.2.4 of the Code, although it appears only in the title of his paper, in which the HYDROBIINAE are classified as a subfamily of the RISSOIDAE. The type genus is only referenced in the stem of the name HYDROBIINAE, but this procedure satisfies the criteria of availability (Article 11.7.1.1 (examples)). Stimpson (August 1865 [b] (pp. 4, 5)) provided a more extensive treatment, including a diagnosis, of the nominal subfamily HYDROBIINAE.

MARGARITIFERIDAE Haas, 1940

The name was placed on the *Official List of Family-Group Names in Zoology* (Opinion 495, *Opinions and Declarations* 17: 287–322, December 1957).

Proposed correction of authorship: Henderson, 1929 (1910) (p. 53).

Henderson (1929) replaced MARGARITANIDAE Ortmann, 1910 with MARGARITIFERIDAE due to the replacement of the name of the type genus *Margaritana* Schumacher, 1817 with its objective synonym *Margaritifera* Schumacher, [1815] ('1816'). According to Article 40.2.1 of the Code, the replacing family-group name takes the date of the replaced name as its date of availability, which should be cited in brackets together with the replacement date (Recommendation 40A).

Genus-group names

The following five generic names are cited in the *Official Index of Rejected and Invalid Generic Names in Zoology*:

Acera Agassiz, 1846 (Opinion 539, *Opinions and Declarations* 20: 65–76, February 1959); *Acera* Agassiz, 1848 (Opinion 539, *Opinions and Declarations* 20: 65–76, February 1959); *Acera* Cuvier, 1810 (Opinion 1079, BZN 34: 16–20, February 1977); *Acera* Lamarck, 1812 (Opinion 539, *Opinions and Declarations* 20: 65–76, February 1959); *Acera* Rafinesque, 1815 (Opinion 539, *Opinions and Declarations* 20: 65–76, February 1959).

Proposed corrections: *Acera* Cuvier, 1810 (corrected page reference: pp. 11, 16), with an added comment that the name is suppressed as an unjustified emendation of *Akera* Müller, 1776. All other entries of '*Acera*' should be deleted.

In Opinion 539, the genus-group name *Acera* has been placed on the *Official Index* four times as an 'Invalid Emendation' of *Akera* O.F. Müller, 1776. This was based on the suggestion made in the original application (Lemche, 1957) that the unjustified emendation '*Acera*' has been introduced independently in the four works quoted. However, Lamarck, 1812 (p. 114) referred implicitly to *Acera* [sensu] Cuvier, and he used only the vernacular term 'Acère' which is not subject to the provisions of the Code. Rafinesque (1815, p. 142) also referred to '*Acera* Cuvier' which he cited as a synonym of his new genus *Agenor* Rafinesque, 1815. In the case of Agassiz's works (1846, 1848), Lemche's conclusion was only based on the absence of express references to earlier publications of the spelling *Acera*, and in any case the 1848 usage is not proposed independently of the 1846 usage by the same author.

The earliest use of the spelling *Acera* instead of *Akera* was by Cuvier, 1810. The name *Acera* Cuvier, 1810 was suppressed in Opinion 1079 as a junior subjective synonym of *Aglaja* Renier, [1807], as if Cuvier had introduced a new genus independent of Müller's *Akera*. It is argued here that Cuvier merely misapplied the name *Akera* Müller, 1776. Cuvier, 1810, treated 'les Acères' as the genus originally proposed by Müller (*Akera*, 1776): 'Je réunis sous le nom générique d'acères, imaginé par Müller, des animaux assez disparates au premier coup d'oeil, quoiqu'ils se ressemblent par tous les caractères essentiels...'. Analysis of the text shows that he included three subgenera, viz. 'bulle Lamarck' [= *Bulla* Linnaeus, 1758 as restricted by Lamarck], 'bullée' [= *Bullaea* Lamarck, 1801] and 'les acères proprement dites'. A single species is included in the last group, *Bulla carnosus* Cuvier 1810 (p. 10, pl. 1 figs. 15–20); this species is also referred to as *acera carnosus* on p. 11, and as *acera* without a species epithet on p. 16. The type species (by monotypy) of *Akera* Müller, *A. bullata* Müller, 1776, is referenced on p. 3 without quoting the species epithet. It would belong to *Bulla* as understood by Cuvier. Cuvier's attribution of the only nominal species of his 'acères proprement dites' to *Bulla* defies explanation and can only be regarded as an error. As Cuvier, 1810, used the spelling *Acera* intentionally rather than erroneously, the name *Acera* Cuvier, 1810 is an unjustified emendation of *Akera* Müller, 1776. This conclusion is based on the fact that Cuvier quoted Müller's name correctly with a 'k', (p. 3), and the spelling with 'c' is consistent with the vernacular 'acère'.

This interpretation of Cuvier's 1810 text is consistent with Cuvier's 1817 (pp. 398–401) presentation. Here he quoted the genus name 'Les Acères (*Akera* Müller)' and mentioned again the three species groups 'bulle', 'bullée' and 'acères proprement dites'. *Doridium* Meckel, 1809 was quoted as a new synonym of the latter group. *Akera bullata* Müller was explicitly included in *Bulla*.

***Balcis* Leach in Gray, 1847**

The name was placed on the *Official List of Generic Names in Zoology* (Opinion 1739, BZN 50(3): 242–243, September 1993).

Proposed correction of authorship: Leach, 1847 (see above).

The name of the type species of *Balcis* was recorded as *Balcis montagui* Leach in Gray, 1847. The authorship of this name should also be changed to Leach, 1847. The name *Balcis montagui* was not placed on any Official List because it is regarded as a junior subjective synonym of *Strombiformis albus* Da Costa, 1778.

***Eucampe* Gray, 1847**

The name was placed on the *Official Index of Rejected and Invalid Generic Names in Zoology* (Opinion 539, Opinions and Declarations 20: 65–76, February 1959).

Proposed correction of authorship: Leach, 1847 (see above).

***Margaritifera* Schumacher, 1816**

The name was placed on the *Official List of Generic Names in Zoology* (Opinion 495, *Opinions and Declarations* 17: 287–322, December 1957).

Proposed correction of date to: Schumacher, [1815] ('1816') (p. 7).

See discussion of the publication date above.

Schumacher's article was reprinted almost literally in 1824 (p. xxxvi). The name *Margaritifera* [which was originally misspelt *Margartifera*] was now misspelt *Margatifera*. Neither has a nomenclatural standing, and the spelling was fixed in Opinion 495 as *Margaritifera*.

***Ocenebra* Gray, 1847**

The name was placed on the *Official List of Generic Names in Zoology* (Opinion 886, BZN 26(3): 128–132, October 1969).

Proposed correction of authorship: Leach, 1847 (see above).

Species-group names

The following species-group name is cited in the *Official Index of Rejected and Invalid Names in Zoology*:

***Eucampe donovani* Gray, 1847**

The name was placed on the *Official Index of Rejected and Invalid Specific Names in Zoology* (Opinion 539, BZN 20: 65–76, February 1959).

Proposed correction of authorship: Leach, 1847 (see above).

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Case 3450

***Chrysophanus florus* Edwards 1884 (currently *Lycaena florus*) (Insecta, Lepidoptera, LYCAENIDAE): conservation of the specific name by designation of a neotype for *Polyommatus castro* Reakirt, 1866 (currently *Lycaena castro*)**

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Abstract. The purpose of this application, under Article 75.5 of the Code, is to conserve the widely used name *Chrysophanus florus* Edwards, 1884 (currently *Lycaena florus*) for a butterfly species from North America by designating a neotype for *Polyommatus castro* Reakirt, 1866. The identity of *Polyommatus castro* (currently *Lycaena castro*) is uncertain because its lectotype is similar to the widespread lowland *Lycaena helloides* (Boisduval, 1852) in one trait and similar to the high-altitude Rocky Mountains butterfly *Lycaena florus* (Edwards, 1884) in another trait, and it lacks precise locality data that could have indicated its identity. A male of *L. helloides* is proposed as the neotype of *L. castro*, which will both fix the identity of *L. castro* as *L. helloides*, a synonymy accepted for 102 years, and conserve the name *Chrysophanus florus* Edwards, 1884 (currently *Lycaena florus*), which has been used for 125 years for the high-altitude butterfly and has been used in most of the scientific papers on the species.

Keywords. Nomenclature; taxonomy; Insecta; Lepidoptera; LYCAENIDAE; *Lycaena*; *Lycaena castro*; *Lycaena florus*; *Lycaena helloides*; *Lycaena dorcas*; coppers; Rocky Mountains.

1. Kirby (1837, p. 299, pl. IV, fig. 1) proposed the name *Lycaena dorcas* for a butterfly in boreal North America.

2. Boisduval (1852, p. 291) proposed the name *Polyommatus helloides* for a widespread North American butterfly.

3. Reakirt (1866, pp. 148–149) proposed the name *Polyommatus castro* for a Colorado butterfly.

4. Edwards (1884, pp. 210–211) proposed the name *Chrysophanus florus* for a Canadian Rocky Mountains butterfly.

5. *Chrysophanus florus* is the long-established name of a butterfly occurring in the higher Rocky Mountains of western Canada and the United States that has generally been considered to be a valid species or subspecies. *Chrysophanus florus* (currently *Lycaena florus*) has been treated as a subspecies of *Lycaena dorcas* by Dyar (1902, p. 41), McDunnough (1922, p. 136), Field (1936, pp. 25–26), Ferris (1977), Bird & Ferris (1979), Miller & Brown (1979, p. 28; 1981), Miller (1981), Bird et al. (1995), Layberry et al. (1998), Koçak & Kemal (2007) and Pelham (2008); as a synonym of *L. dorcas* by Klots (1936, p. 159), dos Passos (1964) and Guppy & Shepard (2001); as

a subspecies of *Lycaena helloides* by Barnes & Benjamin (1926), Leussler (1935, p. 58), McDunnough (1938), Brown et al. (1957, p. 156), Brown (1969), Opler (1975, p. 314) and Scott (1979, 1986, p. 389); and as a variety or form of *L. helloides* by Holland (1931). Chambers (1963) suggested *L. florus* might not be a synonym of either *L. dorcas* or *L. helloides*. Shapiro (1974, p. 40) considered the status of *florus* to be uncertain. Kondla & Guppy (2002, pp. 1–3) and Threatful (2003, p. 11) treated *Lycaena florus* as a valid species. It is no longer considered to be conspecific with either *Lycaena helloides* or *Lycaena dorcas* (Scott et al. 2006; Wright, 2008; Scott, 2006, 2008a, 2008b, 2008c).

6. *Polyommatus castro* was treated as a distinct species after its original description in 1866 and for nine years thereafter (Kirby, 1871; Mead 1875). Then for the next 102 years it was considered to be a synonym of *L. helloides* and so did not appear in local faunal lists and was only cited in synonymy (Scudder, 1876; Strecker, 1878; Dyar, 1902; Barnes & McDunnough 1917; Holland, 1931; McDunnough, 1938; dos Passos, 1964; Tietz, 1972. Barnes & Benjamin (1926) considered that *Polyommatus castro* is a junior synonym of *Lycaena helloides*, and Benjamin even labelled a specimen of *L. helloides* as lectotype of *Polyommatus castro*, but they never published this 'lectotype' labelling. A male paralectotype (Ferris, 1977, fig. 69) of *Polyommatus castro* that is obviously *L. helloides* (identified by Barnes & Benjamin and J. Scott) has a museum label that reads 'Heodes castro Reak Lectotype male Br Benj.' (Ferris, 1977, fig. 69). This label was apparently written by Benjamin when he was William Barnes's curator in 1922–1927 (the red-lined label and its handwriting including the unique ε-like e's are identical to dozens of such labels that also include the year 1925 and initials FHB for Foster Hendrickson Benjamin published by Emmel Emmel & Mattoon, 1998) (the other labels were evidently written by T. Reakirt about 1866).

7. The identity of *Polyommatus castro* Reakirt, 1866 is uncertain. The existing lectotype and four paralectotypes are of two different species, *Lycaena florus* and *Lycaena helloides*. The type series consists of three males and two females (now in the McGuire Center for Lepidoptera, University of Florida, Gainesville, Florida), all illustrated by Ferris (1977, figs. 68–72). The light male paralectotype (fig. 69) and both female paralectotypes (figs. 71, 72) are *L. helloides*, while the dark male paralectotype (fig. 70) is *L. florus* (all identified by J. Scott, and fig. 69 also identified by Barnes & Benjamin as noted above). Ferris (1977, p. 25) validly designated a male specimen, figured in fig. 68, as the lectotype of *Polyommatus castro* (he used the name *Epidemia dorcas castro*), but the identity of this lectotype is ambiguous. It is brightly-coloured like typical *L. helloides*: the blackish border of the brown forewing upperside is narrow like in *L. helloides* whereas it is wider in most *L. florus* (the border width divided by forewing length is the smallest of all five types, the lectotype and four paralectotypes); the blackish border of hindwing upperside (with its conical inward projections into the wide brown center of the wing) is most like *L. helloides*, whereas that border is wider in most *L. florus*; and the underside (especially on the forewing) is brighter, more like *L. helloides*. The number of orange lunules on dorsal hindwing resembles one of the most common variants of *L. florus*, but also resembles some uncommon variants of *L. helloides*. About 10% of *L. helloides* males in La Plata and Routt Counties in Colorado have a similar number of lunules and a similar percentage may have occurred in the mountains at Idaho Springs, Clear Creek Co.,

Colorado where the original collector would have travelled before the habitat was destroyed by construction. But only 1% or fewer males from the plains at Denver, Colorado have that number of lunules. I have seen 14 *L. helloides* that resemble the lectotype of *L. castro* in number of lunules. There are some good characters for distinguishing *L. florus* from *L. helloides*, as noted in para. 9 below, including egg morphology, time of flight during the season, altitude, association with larval hostplants, and electrophoretic alleles, but these characters can not be deduced from old museum specimens with missing locality data. All five specimens (the lectotype and four paralectotypes) are labelled 'P. Castro Reak. Col. [for Colorado] orig. type Coll. Reak.', and the lectotype also has a label 'Polyommatus castro Reakirt Rocky Mts.'. This locality is imprecise, and both *L. florus* and *L. helloides* occur in the Rocky Mountains and in Colorado. Based on historical records of the collector James Ridings (Brown, 1981, p. xii), Scott (2008a) was uncertain where the lectotype of *L. castro* was collected, though he determined that the paralectotype of *L. castro* that is conspecific with *L. florus* might have been collected near Empire, Clear Creek Co., Colorado, and the paralectotypes conspecific with *L. helloides* might have been collected near Denver, Colorado. Ferris chose the lectotype because he believed that this specimen best fitted Reakirt's original description. However, comparison of the type series of *castro* with the original description indicates otherwise, because the lectotype matches Reakirt's original description of males only by the dorsal forewing having dark black spots. Those spots are large both on the upperside and the underside, whereas the original description stated that the spots were much larger on the underside, which is true in the case of the two male paralectotypes representing different species. The original description of *Polyommatus castro* mentioned males with from one to four orange dorsal hindwing lunules, which fits all three males (lectotype and two paralectotypes) that also belong to two species, *L. florus* and *L. helloides*. Both female paralectotypes of *Polyommatus castro* (which are *L. helloides*) match females of *castro* that Reakirt called 'Var. Female' and that have a pale ochraceous dorsal forewing like *helloides*. Neither of these female paralectotypes match the other *castro* females Reakirt described, which may have been lost as they are no longer present in the type series. Without a neotype, some authors may use the name *L. castro* for the high-altitude species, and some may continue to use the name *L. florus* for it. Because the lectotype of *L. castro* is unidentifiable and lacks adequate locality data that could help identify it, a neotype is needed under Article 75.5 of the Code. Unfortunately none of the paralectotypes is suitable for a neotype because all lack adequate locality data.

8. Ferris (1977) considered *castro* to be the high-altitude Southern Rocky Mountains butterfly. His treatment changed the concept of the name *castro*, from a synonym of *Lycaena helloides* to a subspecies of *Lycaena dorcas* conspecific with *florus*. Later authors generally accepted the name *L. dorcas castro* for the Southern Rocky Mountains butterfly (Miller & Brown 1979, 1981; Miller, 1981; Hodges et al., 1983; Koçak & Kemal 2007; Pelham, 2008) and *L. dorcas florus* for the Northern Rocky Mountains butterfly. However, Austin (1998) used the name *L. helloides castro* for the southern butterfly and Scott (2006) used *L. castro castro* for it. Scott (1979, 1986) used the name *L. helloides florus* for the Southern Rocky Mountains taxon and later (2008a, 2008c) used *L. florus* because he doubted the identity of the lectotype of *castro*.

9. Recently the butterflies generally identified as *florus* and *castro* have been considered to belong to a different species than *L. helloides* and *L. dorcas* (Scott et al., 2006; Wright, 2008; Wright & Scott, 2008; Scott, 2008b). The two species *L. florus* and *L. helloides* are sympatric in various locations from British Columbia to Colorado and are distinguished by amount of orange on adults, average darkness of the wings, average size, number of generations per year and month of adult flight, egg sculpturing, larval hairs and larval foods. *Lycaena dorcas* is now considered to be a distinct species in a separate species group, differentiated by egg morphology (Wright, 2008), allozymes (Pratt & Wright, 2003), wing shape, wing pattern and preferred larval foods (Scott, 1979; Scott et al., 2006).

10. Scott (2008a) found that northern Colorado butterflies that Ferris treated as *castro* belonged near the middle of a cline. The butterflies in the Southern Rockies (southern Wyoming through Colorado to northern New Mexico) are not one uniform subspecies as was suggested by Ferris (1977). Scott (2008a) found that the amount of orange on the wings of adult butterflies increases southward in roughly clinal or step-clinal fashion from the Northern Rocky Mountains in Alberta – the type locality of *florus* – and Wyoming, south to northern Colorado – the type locality of *castro* – through southern Colorado to New Mexico, so he proposed a new subspecies *L. florus sangremar* Scott, 2008 (p. 47) (cited as '*L. florus [castro] sangremar*') for the New Mexico end of the cline. Northern Colorado individuals (*castro*) are more similar to those from Alberta (*florus*) than to those from New Mexico (*sangremar*).

11. If a neotype of *castro* conspecific with *L. helloides* were designated by the Commission, New Mexico butterflies would be called *L. florus sangremar*, while Northern Rocky Mountains butterflies and those in northern Colorado near the middle of the cline would be called *L. florus florus*. Thus the name *florus* would continue to be used for the species throughout the Rocky Mountains.

12. If a neotype of *L. castro* conspecific with *L. florus* were designated, New Mexico butterflies would be called *L. castro sangremar*, while those in northern Colorado would be called *L. castro castro*. Northern Rocky Mountains populations would be called *L. castro castro* by persons (such as J. Scott) who accept only two names for a cline, or *L. castro florus* by persons who accept three names. Thus the name *florus*, used for butterflies throughout the Rocky Mountains more than 96 times in 125 years, would be replaced by the less familiar name *castro* (treated as a junior synonym of *L. helloides* until 1977 and thereafter used only for the Southern Rocky Mountains taxon, and since 2006 restricted to the form from the smaller area of northern Colorado), and the usage of *florus* would be limited to a subspecies in the Northern Rockies by persons who can accept three names for a cline.

13. Thus, conservation of the name *florus* (by designation of a specimen of *L. helloides* as the neotype of *castro*) would make a greater contribution to nomenclatural stability than the preservation of the name *castro* (by designating a specimen of *L. florus* as the neotype of *castro*). Neither *florus* nor *castro* has been used very often: 97 usages for *florus* were found (35 listed above, and 62 in a separate list held by the Secretariat) versus 47 usages for *castro* (24 listed above, and 23 in the separate list). However, *florus* dominates in the scientific literature: in major butterfly books there are 5 usages for *florus* versus 1 usage for *castro*; in papers on taxonomic revisions and taxonomic matters 10 vs. 4; in morphology papers 5 vs. 1; in studies of hostplants &

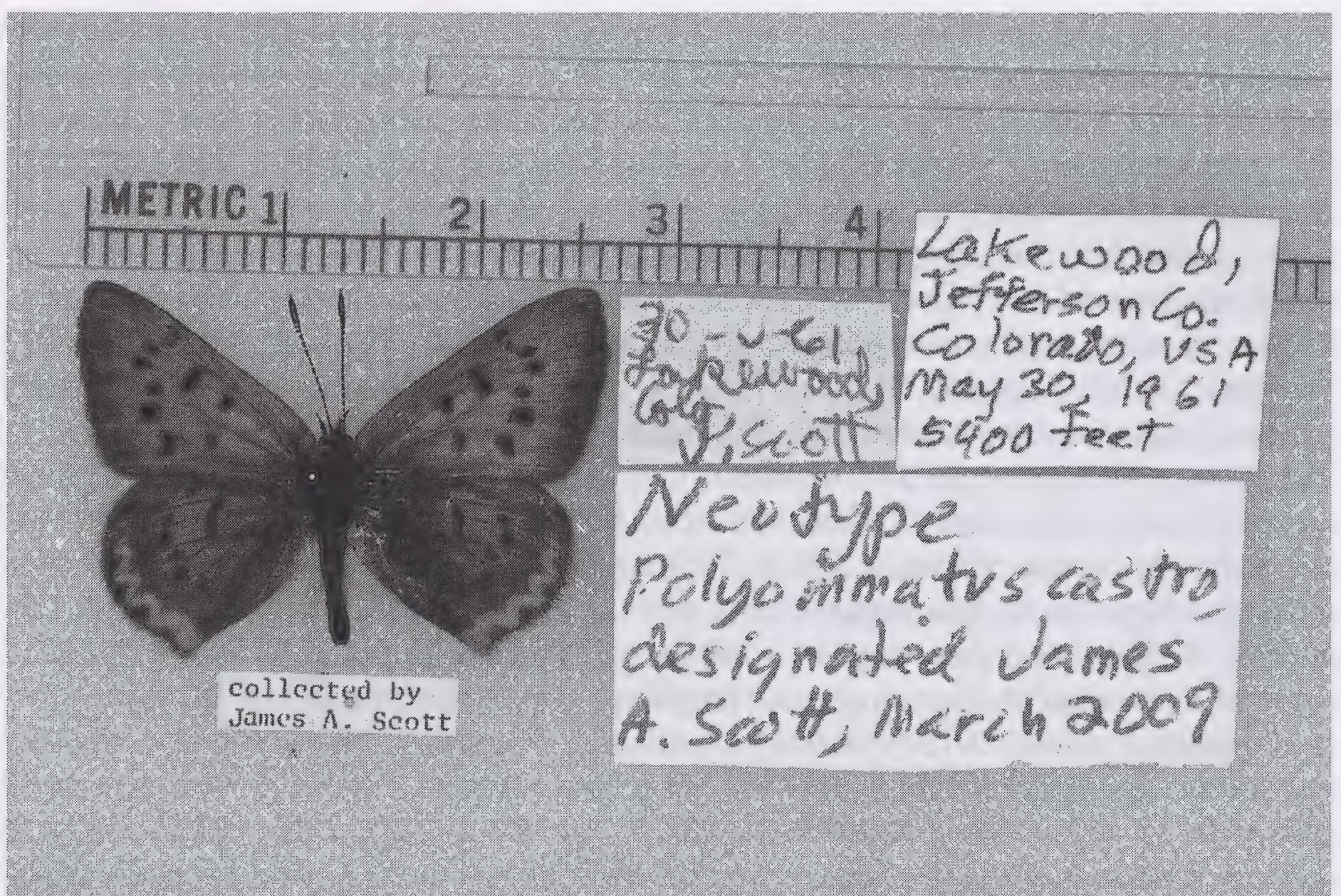


Fig. 1. *Polyommatus castro* Reakirt, 1866. Proposed neotype from Lakewood, Jefferson Co., Colorado, U.S.A.

ecology 4 vs. 0; in geographic distribution 20 vs. 7; in brief citations in checklists 14 vs. 17; it also prevails in internet use (online encyclopedias or museum databases or miscellaneous often non-taxonomic websites) 39 vs. 17. The above examples demonstrate the uninterrupted use of the name *florus* from 1884 to the present, its usage in the entire Rocky Mountains (compared to just the southern Rockies for the name *castro*), and its much greater usage in scientific publications, thereby justifying this application under Article 75.5 of the Code. Most usages of *castro* are simple mentions in checklists or internet databases of little scientific value.

14. It is proposed to designate as neotype a male specimen identified as *Lycaena helloides* from Lakewood (junction of Bayaud Avenue and North Balsam Street), Jefferson Co., Colorado 39°42'57" N, 105°5'16" W, (where its hostplants are *Rumex crispus* and *Polygonum pennsylvanicum*), 5400 feet altitude, collected 30 May 1961 by James A. Scott, to be deposited in the Natural History Museum, London. This locality is the lowest-altitude area presumably visited by the original collector of *Polyommatus castro*.

15. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to set aside all previous type fixations for *castro* Reakirt, 1866, as published in the binomen *Polyommatus castro*, and to designate as the neotype a male specimen identified as *Lycaena helloides* from Lakewood, Jefferson Co., Colorado, to be deposited in the Natural History Museum, London, as specified in para. 14 above;
- (2) to place on the Official List of Specific Names in Zoology the name *helloides* Boisduval, 1852, as published in the binomen *Polyommatus helloides* and as

defined by the neotype designated in (1) above (as the valid name for *Polyommatus castro* Reakirt, 1866).

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Case 3468

***Clusiodes* Coquillett, 1904 (Diptera, CLUSIIDAE): proposed conservation of usage by designation of *Heteroneura albimana* Meigen, 1830 as the type species**

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Abstract. The purpose of this application, under Article 70.2 of the Code, is to conserve usage of the genus *Clusiodes* Coquillett, 1904 (replacement name for *Heteroneura* Fallén, 1823) by designating *H. albimana* Meigen, 1830 as the type species. It has been long overlooked that the type species of *Heteroneura* is actually *H. muscaria* Fallén 1823, a species now assigned to *Cnemacantha* Macquart, 1835 in the family LAUXANIIDAE Macquart, 1835. It is proposed that *H. albimana* be designated the type species of *Heteroneura* in order to maintain the current usage of the name *Clusiodes* for a widespread, regularly collected and frequently discussed taxon in the CLUSIIDAE Handlirsch, 1884.

Keywords. Taxonomy; nomenclature; CLUSIIDAE; *Heteroneura*; *Clusiodes*; druid flies; worldwide distribution.

1. The genus *Heteroneura* was established by Fallén (1823, p. 2) for three new species: *H. muscaria* Fallén, 1823 (p. 2), *H. geomyzina* Fallén, 1823 (p. 2) and *H. scatophagina* Fallén, 1823 (p. 3). No type species was designated. Of these, only *H. geomyzina* is currently classified as CLUSIIDAE; *H. muscaria* is now placed in the LAUXANIIDAE and *H. scatophagina* in the ASTEIIDAE Rondani, 1856.

2. *Heteroneura muscaria* Fallén, 1823 was subsequently designated as the type species of *Heteroneura* Fallén, 1823 by Zetterstedt (1838, column 784) in October 1838 (date from Evenhuis, 1997), but this designation has not been observed outside of that publication.

3. Seven years following Fallén (1823), Meigen (1830, p. 126) described three new species of *Heteroneura*, including *H. albimana* Meigen, 1830 (p. 128) from Switzerland (now known to be widely distributed in Europe and Russia). A redescription of *H. geomyzina* was also included.

4. Presumably unaware of any prior type designation, Curtis (1838, plate 721) designated *Heteroneura albimana* Meigen, 1830 as the type species of *Heteroneura* on 1 December 1838 (date from Evenhuis, 1997). The designation was probably a result of *H. albimana* being the only *Heteroneura* recognised in his publication, which surveyed the insects of Great Britain and Ireland.

5. The designation of *H. albimana* as the type species of *Heteroneura* by Curtis (1838) was accepted by a number of authors, including Westwood (1840, p. 152),

Johnson (1913, p. 97), Melander & Argo (1924, p. 13), Frey (1960, p. 15), Soós (1964, p. 7) and Lonsdale & Marshall (2007, p. 123).

6. Blanchard (1840, p. 628) treated *Heteroneura nubila* Meigen, 1830 (spelled as *Heteronevra Nubila*) as the type species of *Heteroneura*. Lonsdale & Marshall (2007) considered *H. nubila* to be a junior synonym of *H. albimana*.

7. Macquart (1835, p. 587) removed *Heteroneura muscaria* from *Heteroneura* and assigned it to his new genus *Cnemacantha* Macquart, 1835, making it the type species by monotypy. *Cnemacantha* is currently referred to the LAUXANIIDAE, while *Heteroneura* is currently placed in a different family, the CLUSIIDAE.

8. Following this new combination for *Heteroneura muscaria*, Zetterstedt (1848, p. 2787) designated *Heteroneura geomyzina* Fallén, 1823 as the type species of *Heteroneura*.

9. This invalid designation by Zetterstedt (1848) was accepted by Steyskal (1965, p. 807), Sasakawa (1977, p. 236), Soós (1984, p. 11) and Caloren & Marshall (1998, p. 263). However, *Heteroneura albimana*, which was designated as type prior to *H. geomyzina*, is more frequently collected, more frequently discussed, and has been recognised as type by more authors, including the last revisers of the genus, (Lonsdale & Marshall, 2007).

10. When Coquillett (1904, p. 93) realised that *Heteroneura* was preoccupied by a genus earlier described by Fallén (1810, p. 7), he proposed *Clusiodes* as the replacement name for *Heteroneura* Fallén, 1823, which, according to Article 67.8 of the Code (Type species of nominal genus-group taxa denoted by new replacement names), has the same type species of the name it replaced. In this case, the valid type species is *H. muscaria*, making *Clusiodes* a junior synonym of *Cnemacantha* Macquart, 1835 in the LAUXANIIDAE.

11. *Clusiodes*, as currently treated in the literature, comprises 31 described species distributed predominantly throughout the Holarctic Region. It is a regularly collected and frequently cited taxon that has been treated as a clusiid for over a century in at least 44 publications written by over 30 authors from Coquillett (1904) onwards. A record of the history and usage of *Clusiodes* is provided in Lonsdale & Marshall (2007), who recently redefined and revised the genus. A list of references is held by the Secretariat.

12. The International Commission of Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to set aside all previous type fixations for *Heteroneura* Fallén, 1823 and to designate *Heteroneura albimana* Meigen, 1830 as the type species;
- (2) to place on the Official List of Generic Names in Zoology the name *Clusiodes* Coquillett, 1904 (gender: masculine), type species *Heteroneura albimana* Meigen, 1830, as ruled in (1) above;
- (3) to place on the Official List of Specific Names in Zoology the name *albimana* Meigen, 1830, as published in the binomen *Heteroneura albimana* (specific name of the type species of *Heteroneura* Fallén, 1823, and subsequently *Clusiodes* Coquillett, 1904, as ruled in (1) above).

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Case 3476

***Dialictus* Robertson, 1902 and *Evylaeus* Robertson, 1902 (Insecta, Hymenoptera): proposed precedence over *Hemihalictus* Cockerell, 1897, *Sudila* Cameron, 1898 and *Sphecodogastra* Ashmead, 1899**

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Abstract. The purpose of this petition, under Articles 23.9.3 and 81.2.3 of the Code, is to give the widely used halictine bee generic names *Dialictus* and *Evylaeus*, both proposed by Robertson in 1902, precedence over the rarely used but older names *Hemihalictus* Cockerell, 1897, *Sudila* Cameron, 1898 and *Sphecodogastra* Ashmead, 1899 whenever these names are considered to be synonyms. This proposal is intended to best conserve the standard usage of these names.

Keywords. Nomenclature; taxonomy; Hymenoptera; HALICTIDAE; *Dialictus*; *Evylaeus*; *Paralictus*; *Hemihalictus*; *Sudila*; *Sphecodogastra*; *Lasioglossum*; *Lasioglossum* (*Dialictus*) *anomalum*; *Lasioglossum* (*Evylaeus*) *cinctipes*; *Lasioglossum* (*Paralictus*) *cephalotes*; *Lasioglossum* (*Hemihalictus*) *lustrans*; *Lasioglossum* (*Sudila*) *bidentatum*; *Lasioglossum* (*Sphecodogastra*) *texanum*; halictine bees; sweat bees; cosmopolitan.

1. Cockerell (1897a, p. 288) described the genus *Hemihalictus* for a single North American species possessing two submarginal cells and now classified as a halictine. *Panurgus lustrans* Cockerell, 1897b (p. 147) is the type species by original designation (and monotypy). *Hemihalictus* has been in general and continuous use, recently as a subgenus of *Lasioglossum* Curtis, 1833, for the single recognised species, a solitary (Daly, 1961) oligolege on plants of the genus *Pyrrhopappus* DC (Michener, 1947) and related ASTERACEAE in the tribe CICHORIEAE (Arduser, in litt.). Outside of general taxonomic works (e.g. catalogues, keys) *Hemihalictus* has been uncommonly used, typically only in reference to its floral specialisation. Searches for *Hemihalictus* using ISI Web of Knowledge (including Zoological Record) and Google Scholar found 10 and 41 records, respectively.

2. Cameron (1898, p. 52) established the genus *Sudila* to include three Asian halictine species. Sandhouse (1943, p. 602) designated *Sudila bidentata* Cameron (1898, p. 54) as the type species. *Ceylonicola* Friese (1918, p. 501) is a junior subjective synonym of *Sudila*, and the type species *Ceylonicola atra* Friese (1918, p. 502) is a junior subjective synonym of *Sudila bidentata*. The six currently recognised species of *Sudila* were revised as a subgenus of *Lasioglossum* by Sakagami et al. (1996). The name *Sudila* has only been used for a few species with a limited geographic range and only rarely appears in scientific literature. Searches for *Sudila* using ISI Web of Knowledge (including Zoological Record) and Google Scholar found 6 and 9 records, respectively.

3. Ashmead (1899, p. 92) proposed the genus *Sphecodogastra* for a single species of halictine with enlarged ocelli and reduced scopa that specialises on plants of the genus *Oenothera* L. (ONAGRACEAE). *Sphecodes texana* Cresson, 1872 (p. 249) is the type species by original designation (and monotypy). Eight species, all specialists on *Oenothera* and related genera and all matinal, crepuscular or nocturnal, were recognised by McGinley (2003) in his revision of the genus. Most current authors treat *Sphecodogastra* as a subgenus of *Lasioglossum* (e.g. Michener, 2000, 2007). Ebmer (2008) questioned the validity of the taxonomic characters that unite these eight species into a subgenus distinct from *Evylaeus*. Outside of general taxonomic works (e.g. catalogues, keys) *Sphecodogastra* is not commonly used, except in reference to its crepuscular habits and floral specialisation. Searches for *Sphecodogastra* using ISI Web of Knowledge (including Zoological Record) and Google Scholar found 11 and 69 records, respectively.

4. Robertson (1901, p. 299) erected the genus *Paralictus* for three metallic halictine species lacking scopae. The type species was designated as *Halictus cephalicus* Robertson, 1892 (p. 270) but, due to primary homonymy with *H. cephalicus* Morawitz, 1873 (p. 173), the replacement name *Halictus cephalotes* Dalla Torre, 1896 (p. 57) is now used. The group was last revised by Mitchell (1960) who recognised five species. The names *Dialictus* Robertson, 1902 and *Chloralictus* Robertson, 1902 (see below) were given precedence over *Paralictus* in Opinion 1882 (BZN 54: 201–202, September 1997; see also Michener, 1995). *Paralictus* is now most commonly treated as a junior synonym of *Dialictus* (Danforth, 1999; Danforth et al., 2003; Michener, 1995, 2000, 2007) and rarely as a subgenus of *Lasioglossum* (Ebmer, 2002). Outside of general taxonomic works (e.g. catalogues, keys) the name is not commonly used, except in reference to the socially parasitic behaviour of its constituent species. Searches for *Paralictus* using ISI Web of Knowledge (including Zoological Record) and Google Scholar found 8 and 52 records, respectively.

5. Robertson (1902a, Feb. 1, p. 48) established the genus *Dialictus* for a single halictine species with dull metallic integument and two submarginal cells. *Halictus anomalus* Robertson (1892, p. 272) is the type species by original designation (and monotypy). Eight species were recognised by Sandhouse (1923). *Dialictus* is now almost always used to include *Chloralictus* (Mitchell, 1960; Hurd, 1979; Moure & Hurd, 1987; Michener, 2000, 2007; Moure et al., 2007) and in the broadest sense accommodates several hundred species (Michener, 2007) with two, three or variably two or three submarginal cells including non-metallic, ‘acarinat’ *Evylaeus*’ in addition to metallic species (Michener, 2000; 2007). Ebmer (1987, 2002) has maintained that *Dialictus* should be limited to the type species, with the remaining *Dialictus* sensu Michener (2007) in an inclusive *Evylaeus*, a view not shared by any

North American specialist. The name *Dialictus* has been used in hundreds of studies of taxonomy, behaviour, ecology, physiology and biodiversity surveys. Searches for *Dialictus* using ISI Web of Knowledge (including Zoological Record) and Google Scholar found 122 and 946 records, respectively.

6. Robertson (1902b, Sept. 10, p. 247) established the genus *Evyllaesus* for halictine bees related to *Hemihalictus* Cockerell, 1897 with three submarginal cells. The type species is *Halictus arcuatus* Robertson, 1893 (p. 145) by original designation. *Halictus cinctipes* Provancher (1888, p. 316) is a subjective senior synonym of *H. arcuatum*. *Evyllaesus*, as traditionally defined, may be roughly grouped into carinate and acarinate (alternatively referred to as noncarinate or carinaless) forms (Michener, 1990; Packer, 1991). The carinate group includes the type species. Most of the acarinate species have been transferred to *Dialictus* (Michener, 2000; 2007; see also Michener, 1990; 1993; Packer, 1991). This classification is not in use by Old World halictid specialists (Ebmer, 2002; Pesenko, 2007; Murao & Tadauchi, 2007; Pauly et al., 2008). Pesenko (2007) provided a classification of the Palaearctic species of the genus *Evyllaesus* subdivided into 29 subgenera. Pauly et al. (2008) also treated *Evyllaesus* as a genus including three African subgenera all considered synonyms of *Dialictus* by Michener (2007). The name *Evyllaesus* has been used in hundreds of studies of taxonomy, behaviour, ecology and in biodiversity surveys. Searches for *Evyllaesus* using ISI Web of Knowledge (including Zoological Record) and Google Scholar found 154 and 621 records, respectively.

7. Robertson (1902b, Sept. 10, p. 248) erected the genus *Chloralictus* for numerous species related to *Dialictus* with three submarginal cells. *Halictus cressonii* Robertson (1890, p. 317) is the type species by original designation. *Chloralictus* was commonly used (Sandhouse, 1924; Michener, 1944; 1951) until it was synonymised with *Dialictus* by Mitchell (1960). *Chloralictus* is no longer in use, and is treated as a junior synonym of *Dialictus* (Mitchell, 1960; Hurd, 1979; Moure & Hurd, 1987; Michener, 2000, 2007; Moure et al., 2007) or *Evyllaesus* (Ebmer, 1987, 2002; Murao & Tadauchi, 2007). *Chloralictus* has been used infrequently in the last 50 years except when in reference to its relationship to and synonymy with *Dialictus*. Searches for *Chloralictus* using ISI Web of Knowledge (including Zoological Record) and Google Scholar found 23 and 149 records, respectively.

8. Each of the genus-group names mentioned above has been accepted either at the generic rank (Mitchell, 1960; Knerer & Atwood, 1962; Hurd, 1979; Moure & Hurd, 1987; Pauly, 1999; McGinley, 2003; Pesenko, 2007; Moure et al., 2007) or as a subgenus of *Lasioglossum* (Michener, 1944; 1951; 2000; 2007; Ebmer, 2002; Murao & Tadauchi, 2007). Historically, species within *Lasioglossum* sensu lato have been included within *Halictus* Latreille, 1804 but this classification has not been used recently except by Warncke (1975; 1981). The genus-group names of concern here all belong to an informal group called the weak-veined *Lasioglossum* or *Hemihalictus* series as opposed to the strong-veined *Lasioglossum* or *Lasioglossum* series (Danforth, 1999; Michener, 2000, 2007). All available genus-group names within *Lasioglossum* in the broadest possible sense, i.e. all halictine taxa with at least one weakened distal vein of the forewing, are summarised in Table 1.

9. As justified below, the commonly used genus-group names *Dialictus* Robertson, 1902 and *Evyllaesus* Robertson, 1902 should be given precedence over the older names *Hemihalictus* Cockerell, 1897, *Sudila* Cameron, 1898 and *Sphecodogastra* Ashmead,

Table 1. List of genus-group names in order of publication for the *Lasioglossum* group in its broadest sense. The subgeneric names of *Lasioglossum* currently recognised by Michener (2007) are given (except those by Pesenko (2006, 2007) which are inferred based on Pesenko’s categories of carinate (treated as *Evyllaes*) and carinaless or green *Evyllaes* (treated as *Dialictus*); *Warnckenia* is not discussed by Michener (2007) but includes species which have been previously placed in *Lasioglossum* s. str.). *Homalictus* (divided into three subgenera, *Homalictus* s. str., *Papualictus*, *Quasihalictus*), *Echthralictus* (likely a parasitic derivative of *Homalictus*) and *Urohalictus* are treated as genera by Michener (2007) but are herein treated as *Lasioglossum* following Danforth Ji (2001).

Name	Author	Year, page	Vein 1rs-m	Subgenus used by Michener 2007	Type Species	Author	Year	Designation
<i>Lasioglossum</i>	Curtis	1833, pl. 488	strong	<i>Lasioglossum</i>	<i>Lasioglossum tricingulum</i>	Curtis	1833	original
<i>Parasphecodes</i>	Smith	1853, p. 39	strong	<i>Parasphecodes</i>	<i>Parasphecodes hilactus</i>	Smith	1853	Sandhouse, 1943, p. 585
<i>Lucasius</i> ¹	Dours	1872, p. 350	strong	<i>Lasioglossum</i>	<i>Halictus clavipes</i>	Dours	1872	Sandhouse, 1943, p. 566
<i>Hemihalictus</i>	Cockerell	1897, p. 288	absent	<i>Hemihalictus</i>	<i>Panurgus lustrans</i>	Cockerell	1897	original/monotypy
<i>Sudila</i>	Cameron	1898, p. 52	weak	<i>Sudila</i>	<i>Sudila bidentata</i>	Cameron	1898	Sandhouse, 1943, p. 602
<i>Sphecodogastra</i>	Ashmead	1899, p. 92	weak	<i>Sphecodogastra</i>	<i>Sphecodes texana</i>	Cresson	1872	original
<i>Paralictus</i> ²	Robertson	1901, p. 229	weak/ absent	<i>Dialictus</i>	<i>Halictus cephalicus</i> ³ = <i>Halictus cephalotes</i> ⁴	Robertson	1892	original
<i>Dialictus</i>	Robertson	1902, p. 48	absent	<i>Dialictus</i>	<i>Halictus anomalus</i>	Dalla Torre	1896	original/monotypy
<i>Evyllaes</i>	Robertson	1902, p. 247	weak	<i>Evyllaes</i>	<i>Halictus arcuatus</i>	Robertson	1902	original
<i>Chloralictus</i>	Robertson	1902, p. 248	weak	<i>Dialictus</i>	<i>Halictus cressonii</i>	Robertson	1893	original
<i>Gastrohalictus</i> ¹	Ducke	1902, p. 102	weak	<i>Dialictus</i>	<i>Halictus osmioides</i>	Robertson	1890	original
<i>Ctenonomia</i>	Cameron	1903, p. 178	strong	<i>Ctenonomia</i>	<i>Ctenonomia carinata</i>	Ducke	1902	monotypy
<i>Lucasiellus</i> ^{1 5}	Cockerell	1905, p. 272	strong	<i>Lasioglossum</i>	<i>Halictus clavipes</i>	Cameron	1903	monotypy
<i>Nesohalictus</i> ¹	Crawford	1910, p. 120	strong	<i>Ctenonomia</i>	<i>Halictus robbii</i>	Dours	1872	Article 72.2
<i>Halictomorpha</i>	Schrottky	1911, p. 81	weak	<i>Dialictus</i>	<i>Halictomorpha phaedra</i>	Crawford	1910	original
<i>Lucasellus</i> ^{1 5}	Schulz	1911, p. 202	strong	<i>Lasioglossum</i>	<i>Halictus clavipes</i>	Schrottky	1911	original
<i>Prosopalictus</i>	Strand	1913, p. 26	absent	<i>Dialictus</i>	<i>Prosopalictus micans</i> ⁶ = <i>Lasioglossum micante</i> ⁷	Dours	1872	Article 72.2
<i>Ceylonicola</i> ⁸	Friese	1918, p. 501	weak	<i>Sudila</i>	<i>Ceylonicola atra</i>	Strand	1913	original
<i>Curtisapis</i>	Robertson	1918, p. 91	strong	<i>Lasioglossum</i>	<i>Halictus coriaceus</i>	Michener	1993	Sandhouse, 1943, p. 536
<i>Homalictus</i> ¹	Cockerell	1919, p. 13	strong	<i>Homalictus</i>	<i>Halictus taclobanensis</i>	Friese	1918	original
<i>Acanthalictus</i>	Cockerell	1924, p. 184	weak	<i>Acanthalictus</i>	<i>Halictus dybowskii</i>	Smith	1853	original
<i>Echthralictus</i>	Perkins & Cheesman	1928, p. 14	strong	<i>Echthralictus</i>	<i>Halictus extraordinarius</i>	Cockerell	1915	original
<i>Aphalictus</i> ⁹	Cockerell	1930, p. 40	strong	<i>Parasphecodes</i>	<i>Parasphecodes bribiensis</i>	Radoszkowski	1877	original
<i>Indohalictus</i> ¹	Blüthgen	1931, p. 291	strong	<i>Homalictus</i>	<i>Halictus buccinus</i>	Kohl	1908	original
<i>Oxyhalictus</i> ¹	Cockerell & Ireland	1935, p. 91	strong	<i>Ctenonomia</i>	<i>Halictus acufiferus</i>	Cockerell & Ireland	1935	monotypy

Table 1. Continued.

Name	Author	Year, page	Vein 1rs-m	Subgenus used by Michener 2007	Type Species	Author	Year	Designation
<i>Rhynchalictus</i>	Moure	1947, p. 5	weak	<i>Dialictus</i>	<i>Rhynchalictus rostratus</i>	Moure	1947	original
<i>Australictus</i> ¹⁰	Michener	1965, p. 165	strong	<i>Australictus</i>	<i>Halictus peraustralis</i>	Cockerell	1904	original
<i>Pseudochilalictus</i> ¹⁰	Michener	1965, p. 170	strong	<i>Pseudochilalictus</i>	<i>Lasioglossum imitator</i>	Michener	1965	original
<i>Callalictus</i> ¹⁰	Michener	1965, p. 170	strong	<i>Callalictus</i>	<i>Parasphecodes tooloomensis</i>	Cockerell	1929	original
<i>Austrevylaeus</i> ¹⁰	Michener	1965, p. 170	weak	<i>Austrevylaeus</i>	<i>Halictus sordidus</i>	Smith	1853	original
<i>Glossalictus</i> ¹⁰	Michener	1965, p. 173	strong	<i>Glossalictus</i>	<i>Halictus etheridgei</i>	Cockerell	1916	original
<i>Chilalictus</i> ¹⁰	Michener	1965, p. 174	strong	<i>Chilalictus</i>	<i>Halictus subinclinans</i>	Cockerell	1915	original
<i>Microhalictus</i> ¹	Warncke	1975, p. 85	weak	<i>Dialictus</i>	<i>Melitta minutissima</i>	Kirby	1802	original
<i>Puncthalictus</i> ¹	Warncke	1975, p. 87	weak	<i>Dialictus</i>	<i>Hylaeus punctatissimus</i>	Schenck	1853	original
<i>Rostrohalictus</i> ¹	Warncke	1975, p. 88	weak	<i>Dialictus</i>	<i>Halictus longirostris</i>	Morawitz	1876	original
<i>Smeathhalictus</i> ¹	Warncke	1975, p. 88	weak	<i>Dialictus</i>	<i>Melitta smeathmanella</i>	Kirby	1802	original
<i>Pallhalictus</i> ¹	Warncke	1975, p. 92	strong	<i>Lasioglossum</i>	<i>Halictus pallens</i>	Brullé	1832	original
<i>Fahrhalictus</i> ¹	Warncke	1975, p. 95	strong	<i>Lasioglossum</i>	<i>Halictus fahringeri</i>	Friese	1921	original
<i>Marghalictus</i> ¹	Warncke	1975, p. 95	weak	<i>Dialictus</i>	<i>Hylaeus marginellus</i>	Schenck	1853	original
<i>Inhalictus</i> ¹	Warncke	1975, p. 96	weak	<i>Evylaeus</i>	<i>Hylaeus interruptus</i>	Panzer	1798	original
<i>Leuchalictus</i> ¹	Warncke	1975, p. 98	strong	<i>Lasioglossum</i>	<i>Apis leucozonias</i>	Schrank	1781	original
<i>Calchalictus</i> ¹	Warncke	1975, p. 99	weak	<i>Evylaeus</i>	<i>Apis calceata</i>	Scopoli	1763	original
<i>Pyghalictus</i> ¹	Warncke	1975, p. 102	weak	<i>Dialictus</i>	<i>Andrena pygmaea</i> ¹¹	Fabricius	1804	original
<i>Papualictus</i> ¹²	Michener	1980, p. 8	strong	<i>Papualictus</i>	<i>Homalictus megalochilus</i>	Michener	1980	original
<i>Urohalictus</i>	Michener	1980, p. 16	strong	<i>Urohalictus</i>	<i>Urohalictus lieftincki</i>	Michener	1980	original
<i>Sellalictus</i> ⁹	Pauly	1980, p. 120	weak	<i>Sellalictus</i>	<i>Halictus latesellatus</i>	Cockerell	1937	original
<i>Pauphalictus</i> ¹	Warncke	1981, p. 87	weak	<i>Dialictus</i>	<i>Halictus pauperatus</i>	Brullé	1832	original
<i>Labrohalictus</i> ¹⁰	Pauly	1981, p. 719	strong	<i>Ctenonomia</i>	<i>Lasioglossum saegeri</i>	Pauly	1981	monotypy
<i>Habralictellus</i>	Moure & Hurd	1982, p. 46	weak/ absent	<i>Dialictus</i>	<i>Halictus auratus</i>	Ashmead	1900	original
<i>Afrodialictus</i> ¹⁰	Pauly	1984, p. 142	weak	<i>Dialictus</i>	<i>Halictus bellulus</i>	Vachal	1909	original
<i>Mediocralictus</i> ¹⁰	Pauly	1984, p. 143	weak	<i>Dialictus</i>	<i>Halictus mediocris</i>	Benoist	1962	original
<i>Paradialictus</i>	Pauly	1984, p. 691	weak	<i>Paradialictus</i>	<i>Paradialictus synavei</i>	Pauly	1984	original
<i>Lophalictus</i> ¹⁰	Pesenko	1986, p. 125	strong	<i>Lasioglossum</i>	<i>Lasioglossum acuticrista</i>	Pesenko	1986	original
<i>Bluethgenia</i> ¹⁰	Pesenko	1986, p. 136	strong	<i>Lasioglossum</i>	<i>Halictus dynastes</i>	Bingham	1898	original
<i>Ebmeria</i> ¹⁰	Pesenko	1986, p. 136	strong	<i>Lasioglossum</i>	<i>Halictus costulatus</i>	Kriechbaumer	1873	original
<i>Sericohalictus</i> ¹⁰	Pesenko	1986, p. 137	strong	<i>Lasioglossum</i>	<i>Halictus subopacus</i>	Smith	1853	original

Table 1. Continued.

Name	Author	Year, page	Vein 1rs-m	Subgenus used by Michener 2007	Type Species	Author	Year	Designation
<i>Quasilictus</i> ¹²	Walker	1986, p. 166	strong	<i>Quasilictus</i>	<i>Homalictus brevicornutus</i>	Walker	1986	original
<i>Rubrihalictus</i> ¹⁰	Pauly	1999, p. 158	strong	<i>Ctenonomia</i>	<i>Halictus rubricaudis</i>	Cameron	1905	original
<i>Ipomalictus</i> ¹⁰	Pauly	1999, p. 158	strong	<i>Ctenonomia</i>	<i>Halictus nudatus</i>	Benoist	1962	original
<i>Eickwortia</i>	McGinley	1999, p. 112	weak	<i>Eickwortia</i>	<i>Halictus nycteris</i>	Vachal	1904	original
<i>Gnathalictus</i>	Moure	2001, p. 493	weak	<i>Dialictus</i>	<i>Gnathalictus capitatus</i>	Moure	2001	original
<i>Warnckenia</i>	Pesenko	2006, p. 136	strong	<i>Lasioglossum</i>	<i>Melitta quadrinotata</i>	Kirby	1802	original
<i>Minutulaeus</i> ¹³	Pesenko	2007, p. 11	weak	<i>Evyllaes</i>	<i>Hyllaues minutulus</i>	Schenck	1853	original
<i>Nodicornevyllaes</i> ¹³	Pesenko	2007, p. 11	weak	<i>Evyllaes</i>	<i>Halictus nodicornis</i>	Morawitz	1889	original
<i>Biennilaes</i> ¹³	Pesenko	2007, p. 13	weak	<i>Evyllaes</i>	<i>Halictus marginatus</i>	Brullé	1832	original
<i>Monilevyllaes</i> ¹³	Pesenko	2007, p. 13	weak	<i>Evyllaes</i>	<i>Halictus immunitus</i>	Vachal	1895	original
<i>Tricinctevyllaes</i> ¹³	Pesenko	2007, p. 14	weak	<i>Evyllaes</i>	<i>Halictus tricinctus</i>	Schenck	1874	original
<i>Fratevyllaes</i> ¹³	Pesenko	2007, p. 14	weak	<i>Evyllaes</i>	<i>Halictus fratellus</i>	Pérez	1903	original
<i>Malachevyllaes</i> ¹³	Pesenko	2007, p. 15	weak	<i>Evyllaes</i>	<i>Melitta malachura</i>	Kirby	1802	original
<i>Pauxevyllaes</i> ¹³	Pesenko	2007, p. 15	weak	<i>Evyllaes</i>	<i>Hyllaues pauxillus</i>	Schenck	1853	original
<i>Limbevylaeus</i> ¹³	Pesenko	2007, p. 20	weak	<i>Dialictus</i>	<i>Halictus limbellus</i>	Morawitz	1876	original
<i>Crassevyllaes</i> ¹³	Pesenko	2007, p. 20	weak	<i>Dialictus</i>	<i>Halictus crassepunctatus</i>	Blüthgen	1923	original
<i>Laevinodilaes</i> ¹³	Pesenko	2007, p. 20	weak	<i>Dialictus</i>	<i>Halictus laevinodis</i>	Morawitz	1876	original
<i>Pallidevyllaes</i> ¹³	Pesenko	2007, p. 23	weak	<i>Dialictus</i>	<i>Nomioides pallida</i>	Radoszkowski	1888	original/monotypy
<i>Nitidiusculaes</i> ¹³	Pesenko	2007, p. 24	weak	<i>Dialictus</i>	<i>Melitta nitidiuscula</i>	Kirby	1802	original
<i>Truncevyllaes</i> ¹³	Pesenko	2007, p. 24	weak	<i>Dialictus</i>	<i>Halictus truncaticollis</i>	Morawitz	1876	original
<i>Viridihalictus</i> ¹³	Pesenko	2007, p. 25	weak	<i>Dialictus</i>	<i>Halictus viridis</i>	Brullé	1840	original/monotypy
<i>Glauchalictus</i> ¹³	Pesenko	2007, p. 26	weak	<i>Dialictus</i>	<i>Halictus problematicus</i>	Blüthgen	1923	original
<i>Virensalictus</i> ¹³	Pesenko	2007, p. 26	weak	<i>Dialictus</i>	<i>Hyllaues virens</i>	Erichson	1835	original
<i>Loethalictus</i> ¹³	Pesenko	2007, p. 26	weak	<i>Dialictus</i>	<i>Halictus loetus</i>	Brullé	1840	original/monotypy
<i>Aerathalictus</i> ¹³	Pesenko	2007, p. 27	weak	<i>Dialictus</i>	<i>Melitta aerata</i>	Kirby	1802	original

1. Names first proposed for subgenera of *Halictus*. 2. *Dialictus* and *Chloralictus* have priority over *Paralictus* (Opinion 1882, BZN 54: 201–202, September 1997). 3. Not *Halictus cephalicus* Morawitz, 1873. 4. Replacement name *Halictus cephalotes* Dalla Torre, 1896. 5. Replacement name for *Lucasius Dours*, 1872 due to primary homonymy with *Lucasius Kinahan*, 1859. 6. Not *Halictus micans*, Strand, 1909. 7. Replacement name for *Prosopalictus micans* due to primary homonymy with *Halictus micans*, Strand, 1909. 8. Isogenotypic with *Sudila*. 9. Name first described as a subgenus of *Parasphecodes*. 10. Names first described as subgenera of *Lasioglossum*. 11. Identity in doubt (intended for group containing *L. politum* [Schenck]; see Ebmer, 1988, p. 666). 12. Names first described as subgenera of *Homalictus*. 13. Names first described as subgenera of *Evyllaes*.

1899. *Dialictus* Robertson, 1902 and *Chloralictus* Robertson, 1902 were given precedence over the older name *Paralictus* Robertson, 1901 because the former two genus-group names were more broadly applied and because these names were widely used in studies of taxonomy, ecology and behaviour, especially the evolution of social behaviour (Michener, 1995). Nomenclatural issues for this group were not fully resolved by this decision (Michener, 2000, 2007) because of the existence of the additional older names discussed here that are used with, at most, similar frequency to *Paralictus*. The names *Dialictus* and *Evylaeus* have each been used with a frequency more than an order of magnitude greater than the older names. An exhaustive search revealed more records for *Dialictus* in a four year span from 2005 to 2008 than for the combined number of records for *Hemihalictus*, *Sudila*, *Sphecodogastra* and *Paralictus* over more than 100 years. The relatively infrequent use of *Paralictus* was considered suitable justification for giving precedence to *Dialictus* and *Chloralictus* in cases where they are treated as synonymous with the older *Paralictus* (Michener, 1995) but usage of *Paralictus* is greater than that of *Hemihalictus* or *Sudila* and little less than *Sphecodogastra*. Following Michener's (2007) classification, both *Dialictus* and *Evylaeus* contain hundreds of species and are distributed widely in both the New and Old worlds. As a result they are commonly encountered insects which are frequently referred to in scientific papers. In contrast, *Hemihalictus*, *Sudila* and *Sphecodogastra* have only been used to represent species-poor, geographically-restricted taxa which are globally uncommon. The following examples illustrate the wide use of both *Dialictus* and *Evylaeus* in catalogues, keys and taxonomic revisions (e.g. Mitchell, 1960; Knerer & Atwood, 1962; Hurd, 1979; Ebmer, 1987, 1995, 2002; Moure & Hurd, 1987; Michener et al., 1994; Michener, 2000, 2007; Pesenko et al., 2000; Moure et al., 2007; Murao & Tadauchi, 2007; Pesenko, 2007), and studies of ecology and biodiversity (e.g. Leuck & Hammons, 1969; Bernhardt, 1976; Motten, 1986; Wilson & Thomson, 1991; Bishop & Armbruster, 1999; Grixti & Packer, 2006; Giles & Ascher, 2006; Tuell et al., 2009), physiology and genetics (e.g. Hefetz et al., 1978; Duffield et al., 1980; Kukuk & May, 1985; Packer & Taylor, 2002), behaviour and the evolution of social behaviour (e.g. Wilson, 1971, 1975; Sakagami & Michener, 1962; Michener, 1974, 1990; Eickwort, 1988; Packer, 1993, 1994, 1997; Packer & Owen, 1994; Yanega, 1997; Danforth, 1999, 2002; Danforth et al., 2003; Brady et al., 2006; Hirata & Higashi, 2008; see bibliographies for additional references). Given the numerous studies involving the names *Dialictus* and *Evylaeus*, and current application of these names to hundreds of species on all continents where bees occur, stability of nomenclature would be best maintained by setting aside precedence of the less commonly used names *Hemihalictus*, *Sudila* and *Sphecodogastra* under those circumstances when *Hemihalictus*, *Sudila* and *Sphecodogastra* are considered synonymous with *Dialictus* and/or *Evylaeus*.

10. Current usage of the names *Dialictus* and *Evylaeus* results in a nomenclatural problem because these names are beginning to be used in a broader sense that includes the older names *Hemihalictus*, *Sudila* and *Sphecodogastra*. For example, Danforth (1999, 2002), Danforth and Ji (2001), Danforth et al. (2003), and Gibbs (2009) refer to the 'acarinate *Evylaeus*' (species now commonly included in *Dialictus*) which includes both *Sudila* and *Hemihalictus*. However, the taxonomic and nomenclatural implications of this grouping are not discussed. Ebmer (2008) shows that

many of the diagnostic characters of *Sphecodogastra* are evident in species of Palearctic *Evylaeus*, however, he continues to use the latter name as an inclusive group rather than the former. No formal synonymies of these names have yet been made because the rule of priority would preclude the use of the preferred names, *Dialictus* and *Evylaeus* (see Michener, 1979, 1993, 2000, 2007). Revisionary and faunal studies of North American *Dialictus* and *Evylaeus*, which will apply these names in a broad sense to include *Hemihalictus*, *Sphecodogastra* and *Sudila*, are currently in preparation. The revised classification in these studies threatens the long-established application of *Dialictus* and *Evylaeus*. Application of the rule of priority in these cases will require several hundred species of *Dialictus* and *Evylaeus* sensu Michener (2007) to be transferred to *Hemihalictus* and *Sphecodogastra*: decisions unlikely to be accepted by the majority of authors. As in the case of *Paralictus*, these nomenclatural problems would not be completely solved if the precedence of only the oldest name, *Hemihalictus*, was set aside in cases where it is treated as synonymous with *Dialictus* and/or *Evylaeus* because it would then create an identical situation where hundreds of species would be transferred to the infrequently used names *Sudila* or *Sphecodogastra*. Transferring hundreds of species to *Sudila* and *Sphecodogastra*, the next two oldest genus-group names among weak-veined *Lasioglossum*, has the same disadvantages as changing them to *Hemihalictus* or *Paralictus*. For this reason, the precedence of these two names should also be set aside in cases where they are treated as synonymous with *Dialictus* or *Evylaeus*. Applying the rule of priority in these cases would not be in accordance with stability of nomenclature.

11. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary powers to rule that the names *Dialictus* Robertson, 1902 and *Evylaeus* Robertson, 1902 be given precedence over *Hemihalictus* Cockerell, 1897, *Sudila* Cameron, 1898 and *Sphecodogastra* Ashmead, 1899 whenever they are considered to be synonyms;
- (2) to emend the entry on the Official List of Generic Names in Zoology for the name *Dialictus* Robertson, 1902 (gender: masculine), type species by original designation and monotypy *Halictus anomalus* Robertson, 1892, with the endorsement that it is to be given precedence over *Hemihalictus* Cockerell, 1897, *Sudila* Cameron, 1898 and *Sphecodogastra* Ashmead, 1899 in addition to *Paralictus* Robertson, 1901 whenever it and any of the other three are considered to be synonyms;
- (3) to place on the Official List of Generic Names in Zoology the following names:
 - (a) *Evylaeus* Robertson, 1902 (gender: masculine), type species by original designation *Halictus arcuatus* Robertson, 1893, with the endorsement that it is to be given precedence over *Hemihalictus* Cockerell, 1897, *Sudila* Cameron, 1898 and *Sphecodogastra* Ashmead, 1899 whenever they are considered to be synonyms;
 - (b) *Hemihalictus* Cockerell, 1897 (gender: masculine), type species by original designation and monotypy *Panurgus lustrans* Cockerell, 1897, with the endorsement that it is not to be given priority over *Dialictus* Robertson, 1902 or *Evylaeus* Robertson, 1902 when it is considered to be a synonym of either;

- (c) *Sudila* Cameron, 1898 (gender: feminine) type species by designation by Sandhouse (1943) *Sudila bidentata* Cameron, 1898, with the endorsement that it is not to be given priority over *Dialictus* Robertson, 1902 or *Evylaeus* Robertson, 1902 when it is considered to be a synonym of either;
 - (d) *Sphecodogastra* Ashmead, 1899 (gender: feminine), type species by original designation and monotypy *Sphecodes texana* Cresson, 1872, with the endorsement that it is not to be given priority over *Dialictus* Robertson, 1902 or *Evylaeus* Robertson, 1902 when it is considered to be a synonym of either;
- (4) to place on the Official List of Specific Names in Zoology the following names:
- (a) *cinctipes* Provancher, 1888, as published in the binomen *Halictus cinctipes*, senior subjective synonym of *Halictus arcuatus* Robertson, 1898, the specific name of the type species of *Evylaeus* Robertson, 1902;
 - (b) *lustrans* Cockerell, 1897, as published in the binomen *Panurgus lustrans* (specific name of the type species of *Hemihalictus* Cockerell, 1897);
 - (c) *bidentata* Cameron, 1898, as published in the binomen *Sudila bidentata* (specific name of the type species of *Sudila* Cameron, 1898);
 - (d) *texana* Cresson, 1872, as published in the binomen *Sphecodes texana* (specific name of the type species of *Sphecodogastra* Ashmead, 1899).

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Comments on this case are invited for publication (subject to editing) in the Bulletin; they should be sent to the Executive Secretary, I.C.Z.N., c/o Natural History Museum, Cromwell Road, London SW7 5BD, U.K. (e-mail: iczn@nhm.ac.uk).

Case 3483

***Villanyia* Kretzoi, 1956 (Mammalia, Rodentia, ARVICOLIDAE): proposed conservation of usage by designation of a neotype for the type species *Villanyia exilis* Kretzoi, 1956**

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Abstract. The purpose of this application, under Article 75.6 of the Code, is to set aside all type fixations for a species of extinct vole *Villanyia exilis* Kretzoi, 1956 prior to that by Terzea (1991) in order to conserve prevailing usage. The lectotype and paralectotype demonstrably belong to two different taxa. The specimen designated as lectotype by Rabeder (1981) is considered to belong to *Dolomys kretzoi* Kowalski, 1958 (currently *Clethrionomys kretzoi*). The paralectotype corresponds to the original species description and represents a taxon uniformly recognised in the current taxonomic and biostratigraphic literature as *Villanyia exilis* Kretzoi, 1956, which usage should be conserved.

Keywords. Nomenclature; taxonomy; Mammalia; Rodentia; ARVICOLIDAE; *Villanyia*; *Villanyia exilis*; voles; Europe; Pliocene; Pleistocene; biostratigraphy.

1. The new genus and species *Villanyia exilis* Kretzoi, 1956 (p. 65 in the Hungarian text; p. 188 in the German text) was described from the Pleistocene of Hungary (locality Villany-5). There were two syntypes, lower first molars, but no holotype was identified. The description was (transl.) 'A very small sized type (M1- Length 2.0–2.1 mm) combining characters of *Mimomys* and *Prometheomys*, with confluent prism-pairs and wide, rounded, re-entrant folds behind the high *Mimomys* anterior loop (without fold or islet)'. There were no illustrations. These specimens are stored at the Hungarian Geological Survey Museum Budapest (MAFI), under numbers V-12702 VT95#1 and V-12702 VT95#2. The descriptions 'high anterior loop' and 'wide, rounded, re-entrant folds' apply to V-12702 VT95#2 and not V-12702 VT95#1 (Mayhew, in press).

2. Kowalski (1958) described the new species *Dolomys kretzoi* from the Pleistocene of Kadzielnia, Poland. It was later recognised that this species belongs to the clade of red-backed voles *Clethrionomys* Tilesius, 1850 (see e.g. Tesakov, 1996). It has been put forward (Carleton, Musser & Pavlinov, 2003) that *Myodes* Pallas, 1811 is the valid senior synonym of *Clethrionomys* Tilesius, 1850. As this is not yet fully incorporated into working practice, and is not the main point of the current case, the name *Clethrionomys* is used here for clarity.

3. The new genus and species *Cseria gracilis* Kretzoi, 1959 was established based on material from the Pliocene locality Csarnota-2, Hungary.

4. Kretzoi (1969, p. 178) further described *Villanyia*: (transl.) 'The type of this remarkable genus is separated very clearly from [the genus *Mimomys*] by the very long, simple, loop-like anteroconid cap, thick enamel and wide, round re-entrant angles between incompletely separated prisms, and finally by absence of crown cement'. This list of character states serves to clarify the generic and specific description, and applies only to the specimen V-12702 VT95#2 (Mayhew, in press).

5. The use of the generic name *Villanyia* Kretzoi, 1956 was extended from the type species by some authors to include several taxa of fossil voles with rooted teeth, including species of lagurine voles, as reviewed in Gromov & Polyakov (1977). Currently, the majority of authors include these rooted lagurine voles in the genus *Borsodia* Janossy and van der Meulen, 1975 (reviewed by Tesakov, 1993, 2004) leaving in *Villanyia* only a number of (extinct) small forms from the Late Pliocene and Early Pleistocene of Eastern Europe (synonymy in Mayhew, in press). A number of genus level taxonomic issues will be clarified by the neotype designation proposed here, without causing nomenclatural instability.

6. Rabeder (1981, pp. 63–64, figs. 44/1–2) illustrated the two teeth of the *Villanyia exilis* type series for the first time, concluding that they were so different that they belonged to different taxa. He designated one tooth (fig. 44/1, V-12702 VT95#1) as the lectotype of *Villanyia exilis*, and described the other tooth (fig. 44/2, V-12702 VT95#2) as the holotype by monotypy of the new species *Cseria ultima* Rabeder, 1981. However, the descriptions of *Villanyia* by Kretzoi (1956, 1969), noted in paras. 1 and 4, match the specimen illustrated in fig. 44/2 of Rabeder (1981) (V-12702 VT95#2) but not the tooth designated as lectotype by Rabeder (V-12702 VT95#1). The name *Cseria ultima* was not adopted in the literature.

7. Carls & Rabeder (1988, p. 213) stated that the separation of *Cseria ultima* proposed in Rabeder (1981) was not justified, and that the differences they observed between V-12702 VT95#1 and VT95#2 might be considered to fall within the variability of a single species.

8. Terzea (1991) described, under the name *Villanyia exilis* Kretzoi, 1956, abundant material from the Pleistocene of Betfia-XIII, Romania, which agreed in morphology with the tooth from Villany-5 termed *Cseria ultima* by Rabeder (1981, fig. 44/2), i.e. V-12702 VT95#2, the current paralectotype of *Villanyia exilis*. She also (Terzea 1991, p. 91) discussed the lectotype designated by Rabeder (V-12702 VT95#1) and pointed out that this tooth corresponded the least to the original type description of Kretzoi, and was furthermore from a juvenile animal and damaged and resembled *Clethrionomys*. She proposed instead the paralectotype (V-12702 VT95#2) as a replacement lectotype 'la M1 d'adulte ... Rabeder ... figure 44/2'. Terzea (1991, Fig. 3, C) refigured this tooth with a caption stating that it was the lectotype of *Villanyia exilis* viz: 'Lectotype d'après Rabeder, 1981, fig. 44/2'. Terzea (1991, p.100) referred in the text to this tooth as being the lectotype 'calculé sur la fig. 44/2 de Rabeder'.

9. The work of Terzea (1991) is the most complete published description of the teeth of *Villanyia exilis*, covering the entire dentition. Except for Rabeder (1981), the definition of *Villanyia exilis* by Terzea was the basis for the entire current taxonomic and biostratigraphic usage (e.g. Fejfar & Horacek, 1983; Janossy, 1986; Maul, 1990; Kowalski and Nadachowski, 1990; Kordos, 1994; Sala et al., 1994; Aguirre et al.,

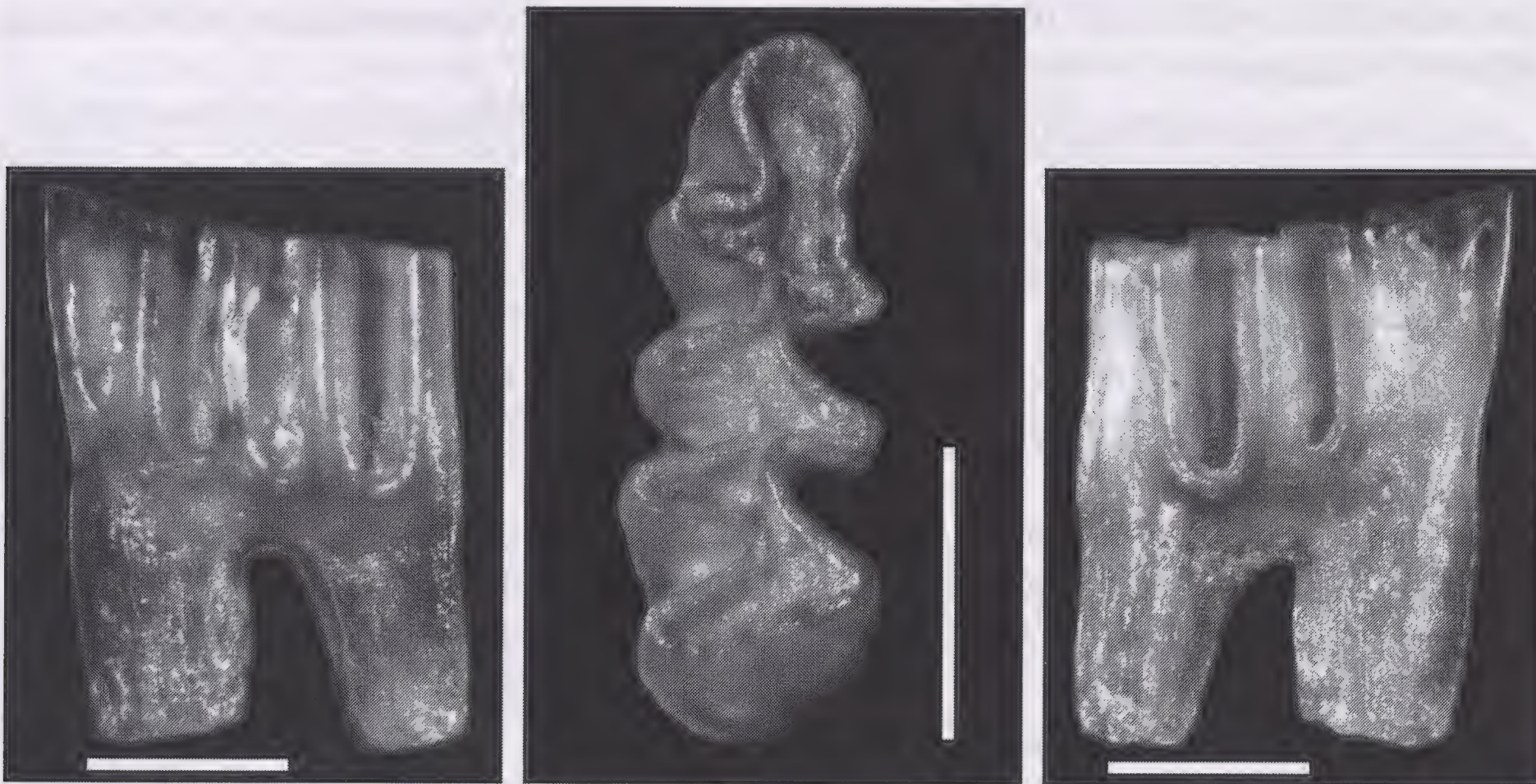


Fig. 1. *Villanyia exilis* Kretzoi, 1956. Proposed neotype GIH V-12702 VT95#2, Museum of the Hungarian Geological Survey (MAFI), Budapest. Lingual, occlusal, and buccal views. Scale bar 1mm.

1997; Nadachowski, 1998; Tesakov, 2004; Maul & Markova, 2007; Popov & Marinska, 2007; Arvibase <http://quarter.ginras.ru/Arvibase/g15.htm>).

10. Tesakov (2004, p. 173) pointed out the resemblance between the lectotype of *Villanyia exilis* selected by Rabeder (1981) (V-12702 VT95#1) and young teeth of *Clethrionomys kretzoi* (Kowalski, 1958).

11. This conclusion is supported by restudy of the material (Mayhew, in press) indicating that the lectotype designated by Rabeder has crown cement and high enamel free areas. These features have not been noted by previous authors and are not indicated in the only illustration in the literature (Rabeder, 1981, fig. 44/1). They confirm that the tooth belongs to the taxon currently known in the literature as *Clethrionomys kretzoi* Kowalski, 1958 and is a different taxon from the paralectotype, which has low enamel-free areas and lacks crown cement.

12. *Villanyia exilis* is the type species of *Villanyia* Kretzoi, 1956. If Rabeder's lectotype designation were to be followed, *Villanyia* Kretzoi, 1956 would become a junior subjective synonym of *Clethrionomys* Tilesius, 1850, which is contrary to the entire usage in the literature; the name *Clethrionomys kretzoi* (Kowalski, 1958) would have to be replaced by *Clethrionomys exilis* (Kretzoi, 1956), which is contrary to the entire usage in the literature; and the species identified as *Villanyia exilis* Kretzoi, 1956 in the current literature would have no generic name, as a new generic name would be needed for *ultima* Rabeder, 1981 which does not belong to the genus *Cseria* Kretzoi, 1959 (Mayhew, in press). This would destabilise the established concept of *Villanyia* Kretzoi, 1956 and *Clethrionomys kretzoi* (Kowalski, 1958) (more than 75 references are deposited with the Secretariat of the Commission). These taxa are referred to in studies of biostratigraphy of the Plio-Pleistocene, also used by other fields such as palaeoanthropology and palaeoecology.

13. It is therefore desirable in the interests of stability and prevailing usage to set aside the lectotype designation by Rabeder (1981) and designate as neotype the

specimen GIH V-12702 VT95#2 in the Museum of the Hungarian Geological Survey (MAFI), Budapest.

14. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to set aside all previous type fixations for *Villanyia exilis* Kretzoi, 1956 and designate specimen GIH V-12702 VT95#2, Museum of the Hungarian Geological Survey (MAFI), Budapest as the neotype;
- (2) to place on the Official List of Generic Names in Zoology the name *Villanyia* Kretzoi, 1956 (gender: feminine), type species by monotypy *Villanyia exilis* Kretzoi, 1956;
- (3) to place on the Official List of Specific Names in Zoology the name *exilis* Kretzoi, 1956, as published in the binomen *Villanyia exilis* (specific name of the type species of *Villanyia* Kretzoi, 1956), as defined by the neotype designated in (1) above;
- (4) to place on the Official Index of Rejected and Invalid Specific Names in Zoology the name *ultima* Rabeder, 1981, as published in the binomen *Cseria ultima* (a junior objective synonym of *Villanyia exilis* Kretzoi, 1956, as defined by the neotype designated in (1) above).

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Case 3480

***Mastodon waringi* Holland, 1920 (currently *Haplomastodon waringi*; Mammalia, Proboscidea): proposed conservation of usage by designation of a neotype**

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Abstract. The purpose of this application, under Article 75.5 of the Code, is to conserve the usage of the name *Mastodon waringi* Holland, 1920, for a species of extinct South American proboscidean, by designating a neotype. Because *M. waringi* has had long and wide (though incorrect) usage as a species of *Haplomastodon*, and because the holotype of *M. waringi* is undiagnostic, the neotype designation is proposed to promote stability of nomenclature.

Keywords. Nomenclature; taxonomy; Mammalia; Proboscidea; Ecuador; Brazil; *Haplomastodon*; *Cuvieronius*; *Mastodon waringi*; gomphothere; South America.

1. In a review of late Cenozoic South American gomphotheres, Hoffstetter (1950, p. 24) named *Haplomastodon* as a new subgenus of *Stegomastodon* Pohlig, 1912. He designated *Masthodon chimborazai* Proaño, 1922 from Ecuador as the type species of *Haplomastodon*. Hoffstetter (1950, p. 24) also noted that the holotype skull of *M. chimborazai* had been acquired by the Central University of Quito, where it had disappeared during the fire of December 1929.

2. Simpson & Paula Couto (1955, p. 11) regarded *Haplomastodon chimborazai* as a junior subjective synonym of *Mastodon waringi* Holland, 1920. Hoffstetter (1955, p. 485) objected to this, noting that *H. chimborazai* was ‘fondé sur un type excellent (crâne complet et divers éléments squelettiques d’un même individu)’, [based on an excellent type (a complete skull and various skeletal elements of a single individual)], whereas *M. waringi* was based on ‘un mauvais matériel fragmentaire’ [bad and fragmentary material].

3. Simpson & Paula Couto (1957, p. 172) also stated that *Mastodon waringi* was the type species of *Haplomastodon*. This is contrary to Article 68.2 of the present Code (Type species by original designation), because Hoffstetter (1950, p. 4) had explicitly designated *Masthodon chimborazai* as the type species of *Haplomastodon*.

4. Nevertheless, all subsequent workers on the taxonomy of South American fossil proboscideans have followed Simpson & Paula Couto (1957) and treated *Haplomastodon waringi* as the type species of *Haplomastodon* (e.g. Parodi Bustos, 1962; Tobien, 1973; Laurito, 1988; Ficcarelli et al., 1993, 1995; Casamiquela et al., 1996; Shoshani & Tassy, 1996; Lucas et al., 1997; Alberdi et al., 2002, 2004; Prado et al., 2002, 2003, 2005; Ferretti, 2008; Lucas, 2008).

5. The type series of *Mastodon waringi* (Carnegie Museum of Natural History), CM 11033, was originally described by Holland (1920) as an incomplete lower jaw

and other fragments collected at Pedra Vermelha in Bahia, Brazil. Holland (1920, fig. 4) only illustrated a molar fragment of the type material (specimen CM 11033f), while Lucas (2008) described and illustrated the entire specimen, which, in 2007, only consisted of molar and tusk fragments. Indeed, Simpson & Paula Couto (1957, p. 172) stated that during the 1950s all that could be located of the holotype were three molar fragments, the tip of a tusk, a tusk dentine fragment and part of the distal end of a tibia.

6. Simpson and Paula Couto (1957, p. 172) considered that CM 11033 could be distinguished from the other genera of South American gomphotheres they recognised, *Notiomastodon*, *Cuvieronius* and *Stegomastodon*, based largely on its geographic origin in eastern Brazil, where the only gomphothere fossils found are *Haplomastodon*. However, recent workers (e.g. Ficarelli et al., 1995; Lucas, 2008) have concluded that the molar and tusk fragments that constitute CM 11033 are not sufficient to identify it as either *Haplomastodon* or *Cuvieronius*.

7. Despite longstanding and wide usage, *Mastodon waringi* is not the correct type species of *Haplomastodon*, and the holotype of *M. waringi* is not diagnostic of either *Haplomastodon* or *Cuvieronius*. Therefore, the species name *M. waringi* is a nomen dubium (Ficarelli et al., 1995; Lucas, 2008).

8. Although the holotype of *Mastodon chimborazai*, a skull, was destroyed in a fire in 1929, it was photographically illustrated and displays cranial features currently considered to be diagnostic of *Haplomastodon* (e.g. Hoffstetter, 1950; Ficarelli et al., 1995; Lucas, 2008). Ficarelli et al. (1995) proposed a neotype for *M. chimborazai*, MECN (Museo Ecuatoriano de Ciencias Naturales, Quito) 82, 83, 84, 133: a skull, lower jaw and partial postcranium of a single individual from Ecuador that they illustrated (Ficarelli et al., 1995, pl. 89, figs. 1–2; pl. 90, figs. 1, 3, 4). This is an appropriate neotype specimen because it preserves anatomical features of the skull, tusks, lower jaw, cheek teeth and cervical vertebrae currently deemed diagnostic of *Haplomastodon*.

9. In proposing a neotype for *Mastodon chimborazai*, Ficarelli et al. (1995) also proposed to invalidate *Mastodon waringi*, as a nomen dubium, and to use *Haplomastodon chimborazai* as a valid name for a species in the genus *Haplomastodon*. This runs contrary to more than 50 years of wide usage prior to Ficarelli et al. (1995), during which time *Haplomastodon waringi* had been recognised as the type and only valid species of *Haplomastodon* (see point 4 above).

10. Instead, in accordance with Article 75.5 of the Code, I propose a solution to possible identity problems associated with *Mastodon waringi* that promotes the stability of nomenclature, i.e. to designate the skull, lower jaw and partial postcranium of a single individual from Ecuador (MECN 82, 83, 84, 133 at the Museo Ecuatoriano de Ciencias Naturales, Quito) as the neotype of *M. waringi* – the same specimen that Ficarelli et al. (1995, pl. 89, figs. 1–2; pl. 90, figs. 1, 3, 4) designated as the neotype of *H. chimborazai*. *H. chimborazai* would retain its status as the type species of *Haplomastodon* (Article 67.1.2 – The name of a type species remains unchanged).

11. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to set aside all previous type fixations for *Mastodon waringi* Holland, 1920 and to designate the skull, lower jaw and partial

postcranium of a single individual from Ecuador (MECN 82, 83, 84, 133 at the Museo Ecuatoriano de Ciencias Naturales, Quito) as neotype of *Mastodon waringi*;

- (2) to place on the Official List of Specific Names in Zoology the name *warangi*, as published in the binomen *Mastodon waringi* Holland, 1920 and as defined by the neotype designated in (1) above.

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**Comment on the proposed conservation of the generic name *Myrmarachne*
MacLeay, 1839 (Araneae, SALTICIDAE)**
(Case 3475; see BZN 66: 20–23)

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We are fully in support of this petition. Drs Dunlop and Penney have thoroughly provided reasons for suppressing the older synonym *Entomocephalus* Holl, 1829. We are in the process of submitting a redescription of the type species of *Myrmarachne*, *M. melanocephala* MacLeay, 1839, and would not like to see this longstanding generic name be supplanted. It also should be pointed out that the fossil (or subfossil) species of Holl cannot be determined to species level in its present state based on modern identification methods (i.e. the species name is meaningless in the sense that, at this time, there is no certain way to assign another specimen to it), and it cannot be sufficiently compared with any extant species or ascertained if it is indeed a recent species of *Myrmarachne* (as the ventral surface of the male palps, the de facto genital organs, cannot be seen). To have *Entomocephalus formicoides* Holl, 1829, as the type species for this genus would preclude any certain inclusion of the 200+ recent species of *Myrmarachne*, as comparison of vital morphological features of the type specimen (even if it was not lost) with other species would be impossible, and the nomenclature of the genus would be thrown into disarray.

**Comment on the proposed conservation of AULACOSCELINAE Chapuis, 1874 (Insecta,
Coleoptera, ORSODACNIDAE or CHRYSOMELIDAE)**
(Case 3398; see BZN 65: 97–105, 66: 72).

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It seems to me that the AULACOSCELINAE / AULACOSCELIDINAE discussion is heading in the wrong direction. In zoological nomenclature it's not a matter of staying faithful to the rules of grammar, but of the stability of nomenclature itself. This is why names can be corrected only when they are incorrect in terms of the Code. Looking through the 'Règles Internationales' (2000 German edition) I can't find any reason why the name AULACOSCELINAE would be incorrect. Thus it is valid and there is no need to correct it, as such a correction will only lead to the creation of a useless synonym.

Comments on the proposed conservation of usage of *Testudo gigantea* Schweigger, 1812 (currently *Geochelone (Aldabrachelys) gigantea*) (Reptilia, Testudines)
(Case 3463; see BZN 66: 34–50, 80–87)

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Taxonomic questions often involve reference to antique documents, but taxonomy is not a religion. It is a blend of scientific observation and application of a consensual, evolving, and complex code of nomenclatural procedures. The present task – i.e. defining the taxon known as *Testudo gigantea* is an example of a purely taxonomic and nomenclatural problem which may secondarily serve as a case history for broader issues. In this case the issue was initiated by non-taxonomists apparently unschooled in the rules of zoological nomenclature and unwilling to abandon a name that they have become used to. They were supported by reviewers who did not follow the series of logical steps and preferred to appeal to fiat authority rather than accept informed scholarship. Disagreements do of course exist among scientists and scholars on many questions, some of which trivial some important. Full, honest, respectful and public discussion of these disagreements does not weaken science, indeed open debate contributes to its progress. But in the case of entrenched major disagreements, it is unacceptable that one group of individuals should claim to be the unique depository of an orthodoxy or 'revealed truth' and demand immunity from challenge. Such problems are not solved by lobbying or conducting a public vote. The ultimate decision should have nothing to do with the number of people sharing an opinion. The history of science is replete with examples where a single person turned out to be correct in the face of an overwhelming 'majority' of people who together disagreed with a lone voice of truth.

One reviewer of our paper was correct in saying that, intellectually speaking, this subject is a minefield. Like literal minefields, it is complete with brave soldiers and cowardly ones. However, those responsible for clearing minefields should not seek to cover them over with opaque material, but rather to deal with the mines one by one so that they present no hazard to future generations.

In conflicted situations in science it is not acceptable that important discussions proceed only behind closed doors, and in such cases 'private discussions' among authors, referees and editors are not a solution. These discussions should be aired publicly and every biology colleague who wishes to contribute to the discussion should be invited to comment on the case, provided that he/she respects the persons with whom he/she disagrees and does not resort to personal attacks or calumnies.

Critics raised a very broad philosophical question: do scientists, in this case taxonomists, have a 'right to commit error,' change their minds, and admit past

mistakes in taxonomic and nomenclatural analyses? And is it possible or acceptable for the perpetrators to correct these errors in the face of new evidence? Are we bound to follow these errors ad infinitum once they have been repeated by a few colleagues? If so, and pushing this situation to its extreme, do we still need a Code of Zoological Nomenclature? Why not just let 'usage', 'consensus' or 'majority' select the binominals and trinominals that should stand? If 'usage' proves to be based on a wrong interpretation of previous texts or type specimens, why not just discard these texts and specimens to protect 'usage'. Then, we no longer need the inconvenient old texts, the old specimens, nor even the museums that conserve them. Such an extreme interpretation may certainly be much appreciated by those people within or outside our governments who think that museums and their staff are very costly and should be terminated. Type specimens would then only be useful when they correspond to current nomenclatural 'usage', but could be 'suppressed' when they do not (as was the case in several recent decisions of the ICZN). Of course, we fully agree that 'usage' should be protected when it is really universal (but this is far from being the case for *Testudo gigantea*), and above all when it concerns not only taxonomists and nomenclaturists, but also laymen, the mass media, general textbooks and so on; i.e., for names like *Drosophila melanogaster*, *Tyrannosaurus rex* or *Homo erectus*. It is clearly acceptable to request that the ICZN invalidate nomina oblita – overlooked, obscure or forgotten senior synonyms of later names that have been in customary use for a long period. However it is an entirely different matter when it is discovered that the holotype of a species is a different taxon from that which it has been presumed to be. It is especially disturbing when the objectors to correction of such situations have apparently not troubled to read the details of the original description, nor examined the actual holotype.

***Testudo gigantea* Schweigger, 1812**

In a key paper describing about two dozen new species of chelonians, Schweigger described a new tortoise, which he named *Testudo gigantea*, the Giant Tortoise (1812, pp. 327; 362). Schweigger (1812, p. 327) added: 'Vidi animal e collectione regi Li[s]bonensi proventum in museo Parisiensi' ('I saw the animal from the King of Lisbon's collection at the Paris Museum'). To summarise the circumstances, Geoffroy Saint-Hilaire had chosen 10 turtle specimens from the King of Portugal's collections in Lisbon to bring to the Paris Museum, where they arrived in mid-November 1808. A tortoise 'de plus de 0m 60' ('longer than 60 cm') was present, as noted on a manuscript list by Lacepède (Daget & Saldanha, 1989, p. 139). It was the type and only specimen of the new species described by Schweigger (Bour, 2006b). One very important point, Schweigger stated precisely and unequivocally 'Habitat in Brasilia' ('Inhabits Brazil').

Pritchard (1986), initially impressed by the stated origin of the Schweigger type specimen ('Brasil'), was convinced that *Testudo gigantea* Schweigger was a synonym of *Testudo denticulata* Linnaeus, 1766 (today *Chelonoidis denticulata*), the (sometimes giant) yellow-footed tortoise of South America which can reach a straight length of 82 cm (Mittermeier in Pritchard & Trebbau, 1984, p. 225). This is a somewhat elongate, narrow-shelled species whose proportions are fully in accord with those indicated by Schweigger in his description. Bour located the type a few years ago, and when later he re-read Schweigger's original text, it became obvious that it was the

very specimen described by this talented student of Constant Duméril. The description and the given measurements leave no doubt about its identity. It must be remembered that the specimen (registered as MNHN 9554) was also described and measured by Duméril & Bibron themselves (1835, p. 89), without any details about its origin, under the heading *Testudo tabulata* Walbaum, 1782, a junior and invalid subjective synonym of *Testudo denticulata*. Its presence in the MNHN collections was further confirmed in a hand-written catalogue dated ca. 1864, with 'Brésil' ('Brazil') as locality (registration number: 120). Pritchard's hypothesis was supported.

***Testudo gigantea* as interpreted by Duméril & Bibron (1835)**

In 1835, Duméril and Bibron associated Schweigger's description with another unique specimen, which was obviously distinct. Duméril & Bibron actually described a new species, but mistakenly attributed it to Schweigger. *Testudo gigantea* sensu Duméril & Bibron (1835, p. 120) has the following features which did not fit with *Testudo gigantea* Schweigger, 1812: 'carapace bombée; écailles du disque très convexes; une écaille nuchale; suscaudale double' ('shell bulged; scutes of the disc very convex; one nuchal [cervical] scute; supracaudal scute double [divided]'). Other details also distinguish this specimen from that described by Schweigger, including the mention of the broad and rough scales of the forelimbs, and the great size: according to Duméril & Bibron, their 'new' *Testudo gigantea* had a shell length (over the curve) of 130 cm and a depth of 49 cm, versus 75.6 cm and 24.3 cm, respectively for the 'old' or Schweigger's specimen. The specimen is still preserved in the Paris Museum collections, with registration MNHN 9566, and it is an Aldabra tortoise.

***Testudo elephantina* Duméril & Bibron, 1835**

The description of *Testudo elephantina* by Duméril & Bibron (1835, p. 110) was based on about eight specimens, from 'Anjouan, Aldebra [sic], les Comores' and Bour (1984a, p. 291), following Rothschild (1915, p. 425), designated as lectotype a large stuffed male (MNHN 7874) on which the description was mostly based. The origin of the species was limited to Aldabra by Günther (1877, p. 18), and the type locality restricted to 'Malabar, Aldabra' by Bour (1984a, p. 291). As outlined by Duméril and Bibron themselves, *Testudo gigantea* was very close to their new species *Testudo elephantina*. This point of view was later shared, for instance, by Günther (1877, p. 22, note) and Boulenger (1889, p. 168). Finally, Rothschild (1897, p. 407), and then Siebenrock (1909, p. 529) combined both nominal species, *Testudo elephantina* being considered as a subspecies of *Testudo gigantea*, but of *Testudo gigantea* sensu Duméril & Bibron! Nevertheless, from the beginning of the 20th century, the valid name for the Aldabra tortoise seemed to have been definitely settled, or at least most often reduced to the nominal species *Testudo gigantea*, and later *Geochelone gigantea*, with Schweigger as author. Both have been widely used up to the present, although *Testudo elephantina* was also regularly used, either as specific or subspecific name, for those who recognised more than one taxon among Seychelles tortoises.

Surely one thing we can all agree upon (assuming that we have all read Schweigger's work) is that the type locality of *Testudo gigantea* is identified as Brazil by the original describer. There are no data to contest this. Whether or not one accepts that MNHN 9554 is indeed Schweigger's type does not change this type

locality; those who insist upon designating a neotype for the species would be bound to select a specimen of a Brazilian tortoise species, of which there are only two. One of them (*Chelonoidis carbonaria*) does not reach the size of Schweigger's specimen. That leaves only *Geochelone denticulata*, of which MNHM 9554 is a conveniently available example.

A neotype for *Testudo gigantea*?

Frazier (2006) strongly emphasized the 'general instability and chaos regarding the valid name of the Aldabra Tortoise'. He favoured an 'established nomenclatural system' (i.e. *Testudo gigantea* according to him; although 'general instability and chaos', and 'established nomenclatural system' are rather subjective), which is a commonly proposed argument – and the only one – against the use of *Testudo dussumieri* Gray, 1831 or *Testudo elephantina* Duméril & Bibron, 1835, the types of which are clearly Aldabra tortoises. Frazier believed that a neotype designation could clarify this situation, and selected a specimen for this purpose, actually more for nomenclatural than taxonomic reasons.

We wish to stress two points stated in the Code (1999) which were not taken into account by Frazier. Recommendation 75B, that 'before designating a neotype, an author should be satisfied that the proposed designation does not arouse serious objection from other specialists in the group in question', was not fulfilled. Furthermore, contrary to the wording of Article 75.3.5, Frazier's neotype is not 'consistent with what is known of the former name-bearing type from the original description' (e.g. absence vs. presence of a cervical scute; limbs shielded by tough and very broad scales vs. only postcranial skeleton, and fragments of skin; from Brazil vs. from Aldabra). Fortunately, the rediscovery of the holotype removes any value from the neotype as the type specimen of the same taxon, so we set aside the neotype according to Article 75.8 of the Code; Frazier's action thus becomes void.

Although aware of the results published by Bour (2006b) regarding the identity of *Testudo gigantea*, Frazier recently (BZN 66: 34–50) decided to request the ICZN to conserve the usage of this name for the Aldabra tortoise under the plenary power. For the reasons given above, we do not support this application.

True, some of the early writers in the field of chelonian systematics were frustrating in the vagueness of their descriptions of new taxa. But Schweigger was not one of these. He was a brilliant and meticulous man, and no arguments have been presented to suggest that his description of *Testudo gigantea* was faulty in any fundamental way. Schweigger wrote that his new species was based upon a large tortoise from Brazil in the King of Portugal's collection in Lisbon, and since the monarch in question had spent the early years of the 19th century (1808–1820) in Rio de Janeiro, where he had received numerous biological specimens collected by Alexandre Rodrigues Ferreira and party (Wilcken, 2004), this is a fully plausible type locality. On the other hand, to assume that Dom João received an Aldabra tortoise (now completely lost) during his stay in Brazil, without a shred of evidence, circumstantial or otherwise, to back this up, is an argument that should be disposed of with a slash from Occam's Razor.

Aldabrachelys* and *Dipsochelys

The remaining question is the choice of the generic name for the Aldabra tortoises and their relatives. *Aldabrachelys*, as a subgenus of *Geochelone* Fitzinger, 1835, was

coined by Loveridge and Williams to include the Aldabra Tortoise and related species, with *Testudo gigantea* Schweigger as type species by original designation (Loveridge & Williams, 1957, p. 225). In fact the discordance between the intended and the actual type specimen of *Testudo gigantea* was not noticed until 1982, when Bour erected a new genus, *Dipsochelys*, with *Testudo elephantina* as type species by original designation, to include the Aldabra tortoise and related species. Bour (1984a, p. 281) was apparently the first to resurrect the nominal species *T. dussumieri* and to recognise its availability, adding 'Perhaps provisionally, we will consider this name as a 'nomen oblitum'.' On the other hand, Gerlach & Canning (1995, p. 133) were certainly the first to coin and use the combination *Dipsochelys dussumieri*; the main justification given being to avoid 'confusion with the phenotypically similar Galápagos complex of *Chelonoidis elephantopus* (Harlan, 1827)', which has since been renamed *Chelonoidis nigra* (Quoy & Gaimard, 1824). Since then, as outlined by Frazier (2006; 2008), Gerlach regularly used *Dipsochelys dussumieri* for the Aldabra tortoise.

Both *Aldabrachelys* and *Dipsochelys* could be considered as being valid candidate names for the Aldabra tortoise. The latter name is nowadays widely used (e.g. Grzimek, 2003; Bonin et al., 2006; Roberts, 2007; Cheke & Hume, 2008; Pedrono, 2008; Vetter, 2008; Wyneken et al., 2008), either as *D. elephantina* or as *D. dussumieri*, and we see no reason not to name the Aldabra tortoise *Dipsochelys dussumieri*.

Aldabrachelys was rarely used until recently; a claim to use *Aldabrachelys* rather than *Dipsochelys* cannot be made on the grounds of stability. Therefore, the genus *Aldabrachelys*, with *Testudo gigantea* as type species, is a junior subjective synonym of *Chelonoidis* Fitzinger, 1835, which has *Testudo boiei* Wagler, 1833 (a junior subjective synonym of *Testudo carbonaria* Spix, 1824) as type species by subsequent designation of Fitzinger, 1843. *Dipsochelys* is the valid genus name for the Aldabra tortoise and its relatives.

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While stability in nomenclature is clearly generally desirable, this utopian expectation should not be allowed to override advances and (re-)discoveries in taxonomy, phylogeny and museum science. In the case under consideration here, Frazier (BZN 66: 34–50) seeks to set aside the established rules of types and priority to attach a particular name to the giant tortoise from Aldabra, a species which has had three well-used specific names over the years. I agree with Frazier that the name should be stabilised, but not with his choice.

Bour (1982, 1984a, b) questioned whether the animal described as *Testudo gigantea* by Schweigger (1812) was an Aldabra tortoise, initially suggesting, on several plausible anatomical grounds, that it was ‘incontestably’ (Bour, 1982) or ‘most probably’ (Bour, 1984a) a Mascarene tortoise and a junior synonym of *Testudo* (currently *Cylindraspis*) *indica* Schneider, 1783, now known to have been endemic on Réunion island (Austin et al., 2002). Pritchard (1986), also recognising that Schweigger’s description did not fit animals from Aldabra, and noting the rather obvious clue that the type was said to come from Brazil, considered it fitted *Testudo* (now *Chelonoidis*) *denticulata* Linnaeus, 1766. At that time the type specimen was missing, but when rediscovered more recently (Bour, 2006; Bour & Pritchard, submitted), it proved to be exactly what Pritchard suspected, a *Chelonoidis denticulata*. Frazier doubts the identity of the rediscovered specimen basically on the grounds that Schweigger and Duméril & Bibron would not have made such a simple misidentification – but in fact tortoise taxonomists at the time regularly did exactly that, as Frazier’s own paragraphs 3, 4, 5 and 8 amply demonstrate. Given the details that Bour & Pritchard (submitted) have added to Bour’s (2006) original paper on the rediscovery, I see no reason to doubt that specimen MHN 9554 is Schweigger’s holotype, and that *Testudo gigantea* is thus a junior synonym of *T. denticulata*; Frazier’s neotype is thus invalidated under Article 75.3.5 of the Code. Thus unless the rules of priority are set aside, *gigantea* cannot continue to be used for the Aldabra tortoise. So the issue is – should the rules be set aside in this particular instance? This should surely only be done if there is an absolutely cast-iron case to preserve a thoroughly established name, the loss of which would cause substantial confusion and upheaval amongst users.

Is *gigantea* then so thoroughly established that it has an unassailable claim to preservation? If it had been the only name in general use for many decades, there might be a case for this, but clearly this is not the position, although there was a

period of active publication on Aldabra during the 1960s and 1970s when *gigantea* was used almost exclusively (e.g. tortoise papers in Stoddart & Westoll, 1979, and references therein). Two other names, *Testudo dussumieri* Gray, 1831 and *Testudo elephantina* Duméril & Bibron, 1835, both explicitly based on specimens from Aldabra (*dussumieri*) or Seychelles generally (*elephantina*) were coined in the 1830s. The latter was frequently used in the 19th century (fide Bour & Pritchard, submitted) as a synonym or variety of '*gigantea*', but this was *gigantea* sensu Duméril & Bibron, 1835, who redescribed '*gigantea*' from a quite different specimen from that used by Schweigger – their specimen survives in Paris as MHN 9566. Gray's type of *dussumieri* seen in Leiden likewise survives – ref. RMNH 3231 (Bour & Pritchard, submitted). In his definitive monograph, Günther (1877) used *elephantina* as one of the four species he believed occurred in the Seychelles and Aldabra, but did not use *gigantea*.

Hence the earliest valid names available are *dussumieri* and *elephantina*. Once Bour (1982; 1984a, b) had queried the identity of *gigantea* with the Aldabra tortoise, and recommended the use of *elephantina* over *dussumieri* on the grounds that the latter was effectively a nomen oblitum, several authors started again using *elephantina* (see Frazier's paragraphs 17 & 19, to which I should add myself (Cheke 1987)). In 1995 Gerlach & Canning (1995) revived *dussumieri*, and Gerlach and his associates have used it regularly ever since (e.g. Gerlach, 2004 and references therein), and its use has spread to other authors (Frazier's paragraph 23) including myself (Cheke & Hume, 2008). Chambers (2004) provided a layman's outline of the taxo-nomenclatural debates. It was perhaps unfortunate that *dussumieri* was revived, but its frequent use over the last 15 years or so means that it can no longer be realistically considered a nomen oblitum, and to suppress it now would be closing the stable door long after the horse (or tortoise?) had departed. I therefore propose that *dussumieri* should be confirmed by the Commission as the correct scientific epithet for the Aldabra tortoise, in accordance with the rules of the Code. It commemorates Jean-Jacques Dussumier, who was an assiduous French zoological explorer and collector in the early-mid 19th century (Laissus, 1973).

One senses from his use of '*Geochelone (Aldabrachelys) gigantea*' that Frazier would also like to preserve *Geochelone* as the generic name for the Aldabra tortoise, a view that is taxonomically untenable in the light of the evident, indeed rampant, polyphyly (e.g. Le et al., 2006; Fritz & Bininda-Emonds, 2007) of the many species until recently included under this name. Hence a new generic name is essential, and the rivals here are *Aldabrachelys* Loveridge & Williams, 1957 and *Dipsochelys* Bour, 1982. Bour established *Dipsochelys* on the grounds that, as *Aldabrachelys* was founded on Schweigger's *Testudo gigantea*, it fell into synonymy with, as he then thought, *Cylindraspis* (Bour, 1982), or as we now know (see above), *Chelonoidis*. However, Loveridge & Williams (1957) explicitly based their subgenus (as was) on the Aldabra tortoise, and it was simply due to the then usage of *gigantea* for that species that the name now falls into synonymy with *Chelonoidis*. Hence, given their perfectly clear intent, I see no serious objection, should the Commission so decide, to preserving *Aldabrachelys* over *Dipsochelys* as the valid genus for the Aldabra tortoise and its two extinct congeners from Madagascar. So doing would also partly satisfy those in the '*gigantea*' camp, as they also invariably prefer *Aldabrachelys* over *Dipsochelys* if unable to use *Geochelone*. However I would equally have no objection

if the Commission preserved *Dipsochelys* and suppressed *Aldabrachelys*. It is however desirable to fix the genus name one way or the other.

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I support the application by Frazier to conserve the usage of *Testudo gigantea* Schweigger, 1812 for the giant land tortoise found on Aldabra Atoll in the western Indian Ocean. This name has been known locally and internationally for several generations and is utilised and established in numerous publications and articles. Having worked on Aldabra for over two years, and as a previous management authority for CITES in Seychelles, I can attest to the high level of recognition given to the specific name *gigantea* globally.

There is a real need for the Commission to stabilise and establish the name for the Aldabra tortoise once and for all so that there is continuity in the scientific and conservation efforts which have taken place over the past hundred years.

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As someone with a long involvement with Indian Ocean reptiles, I support the proposal of J. Frazier to conserve the species name of the Aldabra tortoise as *gigantea* and accept the designation of a neotype.

The present instability, where three species names have recently been used for this species, has many deleterious effects. 1. Scientific nomenclature is brought into disrepute, to the extent that some people have simply abandoned it in the present case and use vernaculars instead. 'Aldabra tortoise' is presently more certain in its meaning and ad hoc status than the competing scientific names, and avoids conflicts and uncertainties about which of these to use. 2. Name instability is bad for conservation of the species concerned. Most, perhaps all, international and national protective legislation uses the species name *gigantea* for the Aldabra tortoise. Any uncertainty about its meaning risks depriving the species of some of its legal protection. 3. Protracted disputes over scientific names such as the present one take up significant amounts of time that would have been more usefully spent on studying and conserving the animals concerned (witness the numerous published papers, comments and petitions in the present case). While arguing at length about nomenclatorial problems has all the appearances of scholarship it does not advance our knowledge of the natural world.

The name *gigantea* is by far the most commonly used and understood for the species, and the one which elicits nearly all the biological information about it when searching databases etc. Consequently, fixing the name *gigantea* for the Aldabra tortoise would help the very large audience of non-taxonomists, most of whom already use the name. It is the needs of these biologists, conservationists, legislators and hobbyists that should be addressed in the present case. What the much smaller number of taxonomists might prefer is far less important, especially as they are used to synonyms. Nomenclature should not merely be a playground for specialists but should address the needs of the majority of users.

If the species name *gigantea* is not fixed for the Aldabra tortoise, instability is likely to persist, as some specialists are unlikely to accept the alternative interpretation, that Schweigger's original description of *Testudo gigantea* really refers to a South American species. The scientific literature of the last twenty years or so shows that the description is open to radically different interpretations. While the case for the recent discovery of the actual type of *T. gigantea* may be credible, by no means everyone involved believes that it is certain.

Fixing *gigantea* as the name of the Aldabra tortoise would also harmonise the case for using the generic name *Aldabrachelys* Loveridge and Williams, 1957 for it. The authors clearly intended *Aldabrachelys* to refer to the Aldabra tortoise and its relatives and clearly believed that the type species of the genus, named as *Testudo gigantea*, was this taxon. Alternative identifications of Schweigger's *T. gigantea* resulted in the creation of the more recent alternative generic name *Dipsochelys* which is presently used alongside *Aldabrachelys* (and also *Geochelone*!) adding to the multiplicity of combinations used for the Aldabra tortoise and the subsequent nomenclatorial confusion.

(5) Peter A. Meylan

Collegium of Natural Sciences, Eckerd College, 4200 54th Ave S., St. Petersburg, FL 33711, U.S.A. (e-mail: meylanpa@eckerd.edu)

I am writing to ask you to stabilise the tortoise genus name *Aldabrachelys* as proposed by Jack Frazier. In 1987, Walter Auffenberg (leading authority on the family TESTUDINIDAE in his day, now deceased) and I published a paper in which we described an extinct *Aldabrachelys* from Tanzania. We did this a few years after Bour offered his alternative name for the giant tortoises of Aldabra. At that time we did not find this necessary and to this day I have continued to use the genus name *Aldabrachelys* for these tortoises. In 1987 we wrote: 'Bour (1982) has recently stated that the specific name *gigantea* is not available for the giant tortoises of Aldabra. He also argues that because the subgeneric (or generic) name *Aldabrachelys* is associated with the name *gigantea*, it cannot be used either. He provides a new name, *Dipsochelys*, for the Seychelles tortoises. The crux of his argument is that Schweigger (1812) had a specimen of *Cylindraspis indica* (Schneider, 1783) in hand when he described *Testudo* (= *Geochelone*) *gigantea*. This is apparently debatable (Crumly, MS). Even if it could be shown with certainty that the name *gigantea* is based on a specimen of *Cylindraspis*, the name *Aldabrachelys* is not necessarily invalid. As stated in Article 70 of the Code of Zoological Nomenclature, when a type species is misidentified the case is to be referred to the Commission on Zoological Nomenclature for consideration. This has not been done. There is no doubt that Loveridge and Williams (1957) applied the name *Aldabrachelys* to those tortoises which today have their centre of abundance on Aldabra Island. Therefore we continue to use this well established and consistently used name.' (Meylan & Auffenberg, 1987, p. 74). More than 20 years later, I think this argument is still valid. I for one have always thought of the name *Dipsochelys* as an unnecessary nuisance.

Additional references

Meylan, P.A. & Auffenberg, D.W. 1987. The chelonians from the Laetoli Beds. Pp. 62–78 in Leakey, M.D. & Harris, J.M. (Eds.), *Laetoli: a Pliocene site in Northern Tanzania*. xxi, 561 pp. Clarendon Press, Oxford.

(6) R. Bruce Bury

US Geological Survey Forest and Rangeland Ecosystem Science Center, 3200 SW Jefferson Way, Corvallis, OR 97331, U.S.A. (e-mail: buryb@usgs.gov)

I read Jack Frazier's Case 3463 to the Commission, where he proposes to conserve the name *Testudo gigantea* for the Aldabra tortoise. I support his major points and arguments. The wide and frequent usage of the name *T. gigantea* needs to be continued. I see no need to confuse the scientific and environmental community with new name combinations. We need to focus our limited time and energies on the protection of this unique life form.

(7) C. Kenneth Dodd, Jr

University of Florida, Department of Wildlife Ecology and Conservation, Gainesville, FL 32611, U.S.A. (e-mail: terrapene600@gmail.com)

I have read the petition submitted by Jack Frazier regarding the Aldabra Tortoise, *Geochelone (Aldabrachelys) gigantea* (BZN 66: 34–50). Frazier has presented an accurate and unbiased summary of the highly complex nomenclatural history of this species. I concur with his conclusion that the many name changes proposed for this species have resulted unnecessarily in immense confusion, particularly among non-tortoise specialists and researchers and administrators working in conservation biology.

I absolutely concur with his position that: 1) USNM 269962 should be designated as the neotype of this taxon; 2) the name *dussumieri* should be suppressed; 3) the names *Aldabrachelys* and *gigantea* should be placed on the Official Lists of Generic and Specific Names in Zoology as specified in the petition; and 4) the name *dussumieri* should be placed on the Official Index of Rejected and Invalid Specific Names, as regards *Testudo dussumieri*.

Only by this action will the Commission be able to stabilise giant tortoise taxonomy in a manner which accurately reflects what is known from original descriptions and type designations. Changes in tortoise nomenclature are counter-productive in advancing clarity, accuracy, and the conservation of this species. In that regard, Frazier is commended for taking the time to exhaustively document Aldabra tortoise nomenclature and for preparing this petition.

(8) Otto Kraus

Zoologisches Institut & Museum, Universität Hamburg, Martin-Luther-King-Platz 3, 20146 Hamburg, Germany (e-mail: Otto.Kraus@zoologie.uni-hamburg.de)

I am acquainted with the case as I provided advisory comments when the application was under preparation. In order to avoid disadvantageous confusion, the well-known and frequently used names *gigantea* Schweigger, 1812 and *Aldabrachelys* Loveridge & Williams, 1957 should be stabilised. In a supplement, the applicant convincingly documented the prevailing usage (between 2000 and 2008).

(9) Colin McCarthy

Department of Zoology, Natural History Museum, Cromwell Road, London SW7 5BD, U.K. (e-mail: C.McCarthy@nhm.ac.uk)

Please can I lend my support to Case 3463 which aims to stabilise the name of the Aldabra tortoise. I believe Jack Frazier has convincingly shown that this is the best resolution to a most confusing nomenclatural problem. It is simply unacceptable to have at least 8 different binomial combinations for this tortoise and the time is long overdue for the situation to be stabilised.

(10) Frauke Fleischer-Dogley, Lindsay Chong-Seng, Nancy Bunbury, Naomi Doak, Lars Kristoferson, Carl Gustaf Lundin, Patrick Lablache, Jeanette Larue, Jeanne Mortimer, Elvina Henriette-Payet, Pierre Pistorius & Rainer von Brandis

Seychelles Islands Foundation, La Ciotat Building, Mont Fleuri, PO Box 853, Victoria, Mahé, Republic of Seychelles (e-mail: sif@seychelles.sc)

Maurice Loustau-Lalanne

Seychelles Tourism Board and Seychelles Islands Foundation

Didier Dogley

Department of Environment, Seychelles and Seychelles Islands Foundation Board of Trustees

Stephen Blackmore

Royal Botanic Garden Edinburgh 20a Inverleith Row, Edinburgh, EH3 5LR and Seychelles Islands Foundation Board of Trustees

David Rowat

Marine Conservation Society Seychelles and Seychelles Islands Foundation Science Committee

Adrian Skerrett

Island Conservation Society and Seychelles Islands Foundation Science Committee

We write on behalf of the Seychelles Islands Foundation (SIF) and the SIF Science Committee in support of the petition by Jack Frazier to the ICZN to stabilise the name of the Aldabra giant tortoise, using the established name *Testudo gigantea* (currently *Geochelone* [*Aldabrachelys*] *gigantea*) and to suppress usage of *Testudo dussumieri* Gray, 1831 (currently *Dipsochelys dussumieri*) as published in the BZN 66(1) March, 2009.

The Seychelles Islands Foundation is a Public Trust which was established in 1979 and is responsible for managing and protecting the two UNESCO World Heritage Sites of Seychelles: Aldabra Atoll and the Vallée de Mai on Praslin. SIF coordinates all research and monitoring conducted at these two sites, including all work on the Aldabra giant tortoise on the atoll.

Within SIF, on Aldabra and in the Seychelles in general, *T. gigantea* (currently *G. gigantea*) is the recognised name used for the Aldabra tortoise. It is most widely used in the literature and appears in SIF official documents. This name has been in continuous use for over 100 years and has been recognised as the oldest name for the species for more than 50 years. Since 1982, when Bour argued that the name should be changed to *Dipsochelys elephantina*, which was followed by another proposal in 2006 to change the name to *Dipsochelys dussumieri*, there has been nomenclatural chaos. The current lack of clarity in the nomenclature has caused and continues to cause great confusion amongst researchers and organisations working with the Aldabra tortoise.

In view of this confusion, it is becoming increasingly necessary to stabilise the nomenclature and fix the name of the Aldabra tortoise. The most straightforward way to achieve this would be to maintain the neotype for *T. gigantea* (USNM 269962) that was designated in 2006, and to suppress *Dipsochelys dussumieri*.

(11) Paolo Casale

WWF Italy (& Marine Turtle Specialist Group IUCN/SSC), Via Po 25c, 00198 Roma, Italy (e-mail: paolo.casale@tiscali.it)

I support the proposal by J. Frazier (BZN 66: 34–50, Case 3463) to conserve the specific name *Testudo gigantea* Schweigger, 1812 for the Aldabra tortoise and to maintain the neotype for *T. gigantea* (USNM 269962) that was designated in 2006. As clearly explained by J. Frazier, *T. gigantea* has been in continuous use for over 100 years and has been recognised as the oldest name for the Aldabra tortoise. In my opinion, to resolve this nomenclatural issue and to put an end to the confusion observed since 1982 would be very important for any research and conservation initiatives on this species and should be done immediately.

(12) Eugene S. Gaffney

Department of Paleontology, American Museum of Natural History, Central Park West and 79th St., New York, NY 10024, U.S.A.
(e-mail: genegaffney373@comcast.net)

I agree completely with the request by the authors of Case 3463 for the conservation of the Aldabra name as argued in that submission. This is hardly a unique example but it is a very obvious one in herpetology and is one of the reasons that the ICZN exists.

(13) Vikash Tatayah and Carl Jones, MBE

Mauritian Wildlife Foundation, Grannum Road, Vacoas, Mauritius
(e-mail: vtatayah@mauritian-wildlife.org)

We have been following the debate over the nomenclature of the Aldabra Giant Tortoise very closely at the Mauritian Wildlife Foundation and, in line with common and accepted local usage in Mauritius and Rodrigues, would support the specific name *gigantea* over other suggestions.

As for the genus, *Geochelone* is currently in widespread use locally. We believe that *Aldabrachelys* has good potential, and would adopt the final decision of the Commission. Please consider the above as the official view of the Mauritian Wildlife Foundation.

(14) Robert P. Reynolds

Biological Survey Unit, USGS Patuxent Wildlife Research Center, National Museum of Natural History, PO Box 37012, Washington, DC 20013–7012, U.S.A. (e-mail: reynolds@si.edu)

I am writing in support of the arguments presented by J. Frazier in Case 3463 to conserve the usage of *Testudo gigantea* Schweigger, 1812 and to suppress *Testudo dussumieri* Gray, 1831. I and my coauthors reviewed this issue thoroughly when we published our ‘Catalog of Type Specimens of Recent Crocodilia and Testudines in the National Museum of Natural History, Smithsonian Institution’ (Reynolds, R.P., Gotte, S.W. & Ernst, C.H. 2007. *Smithsonian Contributions to Zoology*, **626**: 1–49). At that time we were highly skeptical of the supposed rediscovery of the holotype of *Testudo gigantea* Schweigger, 1812 by Bour (2006), and we remain unconvinced of the validity of MNHN 9554 as the holotype of *T. gigantea* Schweigger, 1812. With consideration to the original description of *Testudo gigantea* by Schweigger, the assertion by Bour (2006) of the rediscovery of the holotype is neither particularly convincing nor unequivocal. Because of the uncertainty of the supposed rediscovery of the holotype, and for the sake of nomenclatural stability based on more than a century of usage of *T. gigantea* for the Aldabra tortoise, I encourage the Commissioners to support the petition of Case 3463.

(15) Kim M. Howell

Department of Zoology and Wildlife Conservation, University of Dar es Salaam, P.O. Box 35064, Dar es Salaam, Tanzania (e-mail: kmhowell@udsm.ac.tz)

I find the arguments put forward by Jack Frazier to be convincing. As he notes, *gigantea* is the accepted and most widely used name. My experience within the range of this species and, as a former member of the CITES Animals Committee (Co-representative for Africa), is that *gigantea* is generally in use and recognised. In the African region we often must deal with issues relating to the commercial trade in this species and to have another name in existence simply adds to the confusion and may well provide a loophole for the illegal export of this species under a different name or one not familiar to the authorities who control such matters on the continent.

In my opinion, it is good science, good taxonomic practice, and would make for the best management and conservation of the species to accept Frazier’s arguments and conserve *Testudo gigantea*.

(16) Peter K.L. Ng

Dept of Biological Sciences, National University of Singapore, Science Drive 4, Singapore 117543 (e-mail: dbsngkl@nus.edu.sg)

I agree with the application; the constant 'to-ing and fro-ing' with names, dealing with the fog of history and conjectures about original intent is not helpful. The uncertainties of the old literature and old missing, mislabelled or dubiously labelled specimens will not be easily solved and the debate is rather pointless. Even though I am not a herpetologist here are the facts as I see them: we know there is a species of Aldabra tortoise and we know it is endangered and it needs one unambiguous name. I agree that *gigantea* is the name normally associated with it, and I have seen this in almost all major conservation documents and other papers I have read. The logical thing is therefore to keep this name for the species, recognise the neotype as selected by Frazier, and suppress all other purported type material, even if later shown to be so. This will convey the stability necessary for the biologists to move ahead with the much needed conservation efforts in keeping the species alive. It matters not what Schweigger's (1812) original intent or specimen was. Sentiments and history aside, the name used now and for practical purposes is what matters. The subgenus name is a less clear-cut case, although the name itself suggests its conservation will cause fewer problems – but that is another matter as I suspect genus concepts will change even more in the future.

(17) Ravi Chellam

Country Director, WCS-India Program, No. 269, 5th Main Road, Canara Bank Layout, Kodigehalli, Bengaluru – 560 097, India (e-mail: rChellam@wcs.org)

I write to strongly support the petition by Jack Frazier to conserve the usage of *Testudo gigantea* Schweigger, 1812 by the designation of a neotype. Further, I agree that *Testudo dussumieri* Gray, 1831 should be suppressed.

The arguments of Frazier are logical and well-reasoned, and will result in the least amount of confusion. The most obvious advantage of adopting Frazier's application is the stabilisation of a name which has been in wide and continuous use for more than a century. The need to stabilise the name for the Aldabra tortoise is becoming more and more serious due to the potential risk of extinction. The only extant natural population is on a remote and isolated island and is vulnerable. Climate change models predict sea level changes which represent a real threat to a low lying island such as Aldabra. The usage of *Testudo gigantea* Schweigger, 1812 for the Aldabra tortoise is appropriate and least disruptive, and should be conserved.

(18) Eric P. Palkovacs

School of Biology & Ecology, University of Maine, 5751 Murray Hall, Orono, ME 04473, U.S.A. (e-mail: eric.palkovacs@maine.edu)

I support the conservation of the name *Testudo gigantea* Schweigger, 1812 in reference to the Aldabra tortoise, as proposed by J. Frazier in Case 3463. As

described by Frazier, uncertainty and confusion have bred nomenclatural instability, with non-trivial scientific and conservation implications. This petition simplifies the situation in the most valid and logical possible way. The resulting nomenclatural stability will aid in efforts to communicate about and, ultimately, to conserve this unique surviving lineage.

(19) Justin Gerlach

Nature Protection Trust of Seychelles, PO Box 207, Victoria, Mahé, Seychelles
(e-mail: jstgerlach@aol.com)

Frazier's petition to fix the name of the Aldabra tortoise as *Aldabrachelys gigantea* (incorrectly petitioned as *Geochelone (Aldabrachelys) gigantea*) rests on three points: questions over the status of the type specimen of *Testudo gigantea* Schweigger, 1812, arguments of nomenclatural stability and the wider impacts of nomenclature. The arguments put forward by Frazier are flawed on all three points.

Status of the type specimen of *Testudo gigantea* Schweigger, 1812

The contentious and complicated history of the nomenclature of the Aldabra giant tortoise derives from the misplacing of the type specimen described by Schweigger (1812). Schweigger described a large (but not gigantic) tortoise from Brazil originating from the King of Lisbon's collection. On historical grounds this is unlikely to have been a tortoise from the Indian Ocean, which had been dominated by the French and Dutch since the mid 17th century. Much of the material originally held in Lisbon is South American in origin, reflecting Lisbon's role as a colonial power in South America. Accordingly the origin 'Brasilia' is highly plausible. Furthermore the description given by Schweigger notes three distinctive features: the lack of projecting marginal scutes, the lack of a cervical or nuchal scute and the presence of notably large, thickened scales on the limbs. In contrast the Aldabra tortoise has flared marginals, usually (but not always) has a nuchal scute and has no distinctively enlarged scales on the limbs. However all of these features are highly distinctive in South American tortoises, particularly *Chelonoidis denticulata*. Although the type was misplaced in the Muséum National d'Histoire Naturelle, Paris probably in the 1800s, Bour (2006) describes a specimen which corresponds extremely closely to the type. His account of this specimen leaves no reasonable doubt that it is indeed the type of *Testudo gigantea*. This specimen is also easily identifiable as a South American red-footed tortoise *Chelonoidis denticulata*.

On this basis the original description and the holotype of *Testudo gigantea* are identifiable as *Chelonoidis denticulata* and cannot be applied to the Aldabra tortoise without a fundamental changing of taxonomic history. A very strong case would be needed for such a change to be acceptable.

Nomenclatural stability

As noted by Frazier *Testudo gigantea* was applied to the tortoises on Aldabra from the late 19th century. Frazier cites Hubrecht (1881) as being the first person to associate *T. gigantea* with Aldabra. However, in reality Hubrecht referred the type of *T. dussumieri* (from Aldabra) to *T. gigantea*. This is at best a very tenuous association and the first clear statement that *T. gigantea* could be applied to the tortoises

specifically from Aldabra is that of Rothschild (1915). Other names were explicitly associated with Aldabra from an earlier date: *T. dussumieri* (1831), *T. elephantina* (from 1835) and *T. daudinii* (1896). *T. dussumieri* was overlooked for the next 150 years, and *T. daudinii* was only associated with Aldabra tortoises four times, last in 1967 (Bolau 1896; Rothschild 1915; Wermuth & Mertens 1961; Honegger 1967). *T. elephantina* was applied to the Aldabra population regularly until 1954, followed by a 30 year gap until its reappearance in 1983. Thus *T. gigantea* has only been associated with Aldabra for 94 years, compared to 178 years for *T. dussumieri* and 174 years for *T. elephantina*. *Testudo gigantea* has no claim to priority or to uniquely regular use.

The extent of the current confusion and lack of stability is shown by Frazier's own listing of five generic names (*Aldabrachelys*, *Dipsochelys*, *Geochelone*, *Megalochelys* and *Testudo*) with 7 combinations used in the past decade and a total of 9 names in the past two decades. Frazier gives a list of 31 citations in support of the continued use of *gigantea* since 1986, but only 11 of these have any connection to taxonomy, the others mentioning Aldabra tortoises only in passing. The same is true of the statements relating to non taxonomic aspects of biology; the majority of citations concern other species and the name used in their passing reference to Aldabra tortoises is essentially irrelevant. Frazier's list is also misleading in that it does not consider the number of citations of other names. A wider comparison shows that over the past 10 years, peer-reviewed publications including discussion of the taxonomy of Aldabra tortoises have used *gigantea* 4 times and *dussumieri* 9 times. In the same time period non-taxonomic papers (ecology and behaviour) including Aldabra tortoises have used *gigantea* 9 times and *dussumieri* 7 times. This means that in the total scientific literature of the past decade no name has had significantly greater currency or stability, with *gigantea* being used 13 times and *dussumieri* 16 times. Frazier (2006) himself noted the 'general instability and chaos regarding the valid name of the Aldabra Tortoise' and this is borne out by the present analysis. There is no stability to protect.

Wider impacts of nomenclature

Although Frazier is correct in noting that international bodies and conventions use *Geochelone gigantea* for the Aldabra tortoise this has little practical relevance; the tortoises referred to are explicitly the Aldabra tortoises, for which there is no significant identification issue whatever name is applied. Of the three citations given for the use of *G. gigantea* in Seychelles government documents, one is primary legislation specifically concerning tortoises, the other two have no relevance to tortoises and only refer to tortoises as examples of the biodiversity of the islands. Other examples can be cited where different names have been used, for example the Seychelles Biodiversity Strategy and Action Plan (Republic of Seychelles 1997) uses *Dipsochelys* (without a species name) and the reports of the Seychelles Islands Foundation which manages Aldabra have variously used *Geochelone gigantea*, *Testudo gigantea* and *Dipsochelys dussumieri* (e.g. Betts, 2000).

A further implication of this nomenclatural issue is the unintended consequence of potentially validating a misleading name. In 1957 Loveridge & Williams created *Aldabrachelys* as a subgenus of *Geochelone*, designating *Testudo gigantea* as the type species. This name was rejected by Bour (1982) on the basis of the misidentification

of *T. gigantea*. Rejection of *gigantea* required the rejection of *Aldabrachelys* and the creation of a new generic name, *Dipsochelys*. Since then *Aldabrachelys* has been very rarely used (only twice in the past decade in relevant systematic literature) and *Dipsochelys* remains the most widely applied distinct generic name for the Madagascar-Seychelles-Aldabra tortoises. Validation of *gigantea*, as proposed by Frazier, would also validate *Aldabrachelys* as the generic name based on *gigantea*. This is highly undesirable as it would require the adoption of a currently rarely used name, and would apply to all giant tortoises (living and extinct) from Madagascar, Seychelles and Aldabra, not only to the Aldabran population. The name *Aldabrachelys* is very unfortunate in that it clearly ties the genus to Aldabra; whilst Aldabra supports the largest wild population of giant tortoises in the world it has only been occupied by giant tortoises for the past 115,000 years, compared to some 10 million years for Madagascar. There is little doubt that the genus originated on Madagascar and has only a recent history on Aldabra. To promote *Aldabrachelys* over *Dipsochelys* would have a confusing effect on interpretations of a substantial part of the evolutionary history of the genus. Whilst this is not a taxonomic point, it would be very unfortunate for public education and comprehension of nomenclature.

In conclusion, the original description and holotype of *Testudo gigantea* demonstrate that this name applies to the South American *Chelonoidis denticulata*. Setting aside the existing holotype specimen in favour of the neotype proposed by Frazier would be a significant nomenclatural act and should only be undertaken with a strong justification and unequivocal support. The claim that *Testudo gigantea* is a stable name cannot be justified as even Frazier notes that there has not been any stability in the nomenclature of the Aldabra tortoise for the past 27 years. Although Frazier states that his neotype designation was undertaken after 'after extended consultation with numerous specialists in chelonian systematics' he did not include any of the specialists who have worked specifically on the nomenclature of the Aldabran tortoises in the past 25 years, all of whom would have been expected to urge against proposal of a neotype and this petition. Accordingly I recommend that the International Commission on Zoological Nomenclature reject the petition of Case 3463 and allow the Code to operate, validating *Testudo dussumieri* Gray, 1831 (now *Dipsochelys dussumieri*) as the valid name for the Aldabra giant tortoise and retaining *Testudo gigantea* Schweigger, 1812 as a junior synonym of *Chelonoidis denticulata* (Linnaeus, 1766).

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Comment on the proposed conservation of usage of *Cetiosaurus* Owen, 1841 by designation of *Cetiosaurus oxoniensis* Phillips, 1871 as the type species (Case 3472; see BZN 66: 51–55)

(1) Paul M. Barrett

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I write in support of Upchurch et al.'s proposal to establish *Cetiosaurus oxoniensis* Phillips, 1871 as the type species of *Cetiosaurus* Owen, 1841 and to set aside all previous fixations of type species for this genus.

Cetiosaurus was the first sauropod dinosaur to be scientifically described (Owen, 1841) and one of the earliest dinosaurs to be recognised: the taxon is clearly of historical importance and stabilising its taxonomy would represent an important contribution to dinosaur studies. The name has been dogged by nomenclatural instability since its inception due to the proliferation of species names associated with this genus and the large amount of undiagnostic material allocated to it throughout the nineteenth and twentieth centuries (Upchurch & Martin, 2003). As noted elsewhere, many of the early species names are nomina nuda, nomina dubia, junior objective synonyms of other species or referable to taxa (such as *Pelorosaurus brevis* and '*Cetiosaurus*' *glymptonensis*) that are only distantly related to the specimens that are widely acknowledged as belonging to *Cetiosaurus* (Upchurch & Martin, 2003).

Upchurch et al. note that *Cetiosaurus medius* Owen, 1842b is the type species of *Cetiosaurus* under Article 69.1.1 of the Code and that this species had been recognised as the type species by various authors, including Owen (1842b) and Steel (1970). They go on to demonstrate, however, that most published discussions on *Cetiosaurus* tend to ignore the *C. medius* type series, which is fragmentary and of limited utility (the species is currently regarded as a nomen dubium: Upchurch & Martin, 2003), and concentrate instead on the more complete and diagnostic specimens referred to *C. oxoniensis* Phillips, 1871. Consequently, Upchurch et al. argue that fixation of the name *C. medius* as the type species creates confusion and instability, even though it is the correct type species under the Code. As the name *Cetiosaurus* is i) well-established and deeply embedded in the literature, ii) intimately associated with the material comprising the type and referred specimens of the species *oxoniensis* and iii) not generally associated with the material assigned to the species *medius*, it would be desirable to set aside the previous, infrequently cited, type species fixation for this genus and to replace it with one based on a familiar and taxonomically determinate set of material. Conversely, if *Cetiosaurus medius* were retained as the type species of *Cetiosaurus*, the genus would be rendered invalid and a new generic name would be required for *C. oxoniensis*. This would lead to increased confusion and taxonomic instability. Such a situation should be avoided as *Cetiosaurus* is not only a historically important taxon, but also one that has been used to specify other groups within Dinosauria, including Cetiosauridae. In addition, Ornithischia, one of the major dinosaur sub-groups, has been defined as all dinosaurs that are more closely related to *Iguanodon* than they are to *Cetiosaurus* (Norman et al., 2004).

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(2) Peter M. Galton

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I strongly support the proposal by Upchurch et al. (BZN 66(1): 51–55) for the conservation of usage by designating *Cetiosaurus oxoniensis* Phillips, 1871 as the type species of *Cetiosaurus* Owen, 1841. As extensively referenced in the proposal, the name *Cetiosaurus* has invariably been associated with the species *C. oxoniensis*, and specifically the Bletchington Station material, for almost 125 years. In particular, it should be noted that the ‘Monograph of the genus *Cetiosaurus*’ by Owen (1875) is based almost entirely on the Bletchington Station material of *C. oxoniensis* (Owen even used Phillips’ figures!). Also, as noted by Galton & Knoll (2006), the family CETIOSAURIDAE Lydekker, 1888 is based on *C. oxoniensis* Phillips, 1871 because Lydekker (1888, p. 137) indicated it as being the type species of *Cetiosaurus* Owen, with *C. medius* Owen and *C. longus* Owen (in part) as (?) synonyms. This proposal would make Lydekker’s indication official, an action that is long overdue. Although much less complicated, this proposal is similar to that involving the designation of a new type species for another classic genus of Dinosauria, viz., *Iguanodon* (Case 3037; see BZN 55: 99–104, Opinion 1947, BZN 57: 61–62).

Comment on the proposed precedence of *Procynosuchus* Broom, 1937 (Therapsida, Cynodontia) over *Cyrbasiodon* Broom, 1931 and *Parathrinaxodon* Parrington, 1936 (Case 3431; see BZN 66: 64–69)

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I am writing to strongly endorse the application to give precedence to *Procynosuchus* over *Cyrbasiodon* and *Parathrinaxodon*. As Kammerer and Abdala note, the name *Procynosuchus* is widely used and a very important name to conserve because of its significance in evolutionary studies and museum exhibits. The authors have amply documented the preponderance of its usage and strong support within the specialist community.

OPINION 2229 (Case 3405)***Trigonostomum* Schmidt, 1852 (Platyhelminthes, TRIGONOSTOMIDAE) and *Trigonostomus* Brenske, 1893 (Coleoptera, SCARABAEIDAE): generic names conserved**

Abstract. The Commission has ruled that the generic name *Trigonostomum* Burmeister, 1844 for a scarabeid beetle be suppressed, and the incorrect subsequent spelling *Trigonostomus* Brenske, 1893 is deemed available. The Commission has also ruled the entire generic name *Trigonostomus* Brenske, 1893 should be adopted as the stem, so that the correct spelling of the chafer subtribe name will become TRIGONOSTOMUSINA Ohaus, 1912 to remove homonymy with the flatworm family-group name TRIGONOSTOMIDAE Graff, 1905. This conserves the widespread usage of the generic name *Trigonostomum* Schmidt, 1852 for a group of marine flatworms.

Keywords. Nomenclature; taxonomy; Platyhelminthes; Coleoptera; Rhabdocoela; TRIGONOSTOMIDAE; HYPORHYNCHINAE; SCARABAEIDAE; TRIGONOSTOMUSINA; *Trigonostomum*; *Trigonostomus*; *Trigonostomum setigerum*; *Trigonostomus mucoreus*; marine free-living flatworms; scarab beetles.

Ruling

- (1) Under the plenary power it is hereby ruled that:
 - (a) the generic name *Trigonostomum* Burmeister, 1844 and all uses of the name are suppressed for the purposes of both the Principle of Priority and the Principle of Homonymy;
 - (b) the generic name *Trigonostomus* is deemed available from Brenske, 1893 and is not invalid by reason of being an incorrect subsequent spelling of *Trigonostomum* Burmeister, 1844;
 - (c) *Trigonostomum mucoreum* Burmeister, 1844 is designated as the type species of *Trigonostomus* Brenske, 1893;
 - (d) the stem of the generic name *Trigonostomus* Brenske, 1893, as deemed available in (1b) above, is TRIGONOSTOMUS-.
- (2) The following names are placed on the Official List of Generic Names in Zoology:
 - (a) *Trigonostomum* Schmidt, 1852 (gender: neuter), type species by monotypy *Trigonostomum setigerum* Schmidt, 1852;
 - (b) *Trigonostomus* Brenske, 1893 (gender: masculine), type species *Trigonostomum mucoreum* Burmeister, 1844, as designated in (1c) above.
- (3) The following names are placed on the Official List of Specific Names in Zoology:
 - (a) *setigerum* Schmidt, 1852, as published in the binomen *Trigonostomum setigerum* (specific name of the type species of *Trigonostomum* Schmidt, 1852 and of *Hyporhynchus* Graff, 1882);
 - (b) *mucoreum* Burmeister, 1844, as published in the binomen *Trigonostomum mucoreum* (specific name of the type species of *Trigonostomus* Brenske, 1893).

- (4) The following names are placed on the Official List of Family-Group Names in Zoology:
 - (a) TRIGONOSTOMIDAE Graff, 1905, type genus *Trigonostomum* Schmidt, 1852 (Platyhelminthes);
 - (b) TRIGONOSTOMUSINA Ohaus, 1912, type genus *Trigonostomus* Brenske, 1893 (spelling emended by ruling in (1d) above) (Coleoptera).
- (5) The name *Trigonostomum* Burmeister, 1844, as suppressed in (1a) above is hereby placed on the Official Index of Rejected and Invalid Generic Names in Zoology.
- (6) The name TRIGONOSTOMINA Ohaus, 1912 (an incorrect original spelling of TRIGONOSTOMUSINA, as ruled in (1d) above) is hereby placed on the Official Index of Rejected and Invalid Family-Group Names in Zoology.

History of Case 3405

An application to conserve the widespread usage of the generic name *Trigonostomum* Schmidt, 1852 for a group of marine flatworms (family TRIGONOSTOMIDAE) and to remove the homonymy between the chafer subtribe name TRIGONOSTOMINA Ohaus, 1912 (type genus *Trigonostomum* Burmeister, 1844; family SCARABAEIDAE) and the flatworm family name TRIGONOSTOMIDAE Graff, 1905 (type genus *Trigonostomum* Schmidt, 1852) was received from Wim R. Willems (*Swedish Museum of Natural History, Stockholm, Sweden & Centre for Environmental Sciences, Hasselt University, Diepenbeek, Belgium*) and Frank-Thorsten Krell (*Denver Museum of Nature & Science, Denver, CO, U.S.A*) on 17 November 2006. After correspondence the case was published in BZN 64: 218–223. The title, abstract and keywords of the case were published on the Commission's website. One supportive comment on this case was published in BZN 65: 216.

Decision of the Commission

On 1 December 2008 the members of the Commission were invited to vote on the proposals published in BZN 64: 220–221. At the close of the voting period on 1 March 2009 the votes were as follows:

Affirmative votes – 17: Bogutskaya, Bouchet, Brothers, Fautin, Grygier, Halliday, Krell, Kullander, Lamas, Lim, Minelli, Ng, Pape, Papp, Rosenberg, van Tol and Zhang.

Negative votes – 2: Alonso-Zarazaga and Štys.

Kottelat, Patterson and Pyle were on leave of absence.

Alonso-Zarazaga, voting AGAINST, said that, in his opinion, this application had several problems: (1) the authors did not give a list of the species originally included by name in *Hyporhynchus* Graff, 1882, so it was not possible to know if the designation of *Trigonostomum setigerum* Schmidt, 1852 as its type species is valid; (2) he could not find any advantage in changing a name that has been consistently in use in Coleoptera since 1844, because of the incorrect use of a homonym in Platyhelminthes from 1852. In this case, the incorrect use would needlessly supersede the correct one; and (3) the use of *Trigonostomum* Schmidt, 1852 is not overwhelming and *Trigonostomum* Burmeister, 1844 has been in use during the 20th century. Thus, in his opinion, the necessary change was on the side of the platyhelminth name to keep the Principle of Priority: the platyhelminth genus should become *Hyporhynchus*

Graff, 1882 and the family name (which has priority) should be HYPORHYNCHINAE Graff, 1882. Alonso-Zarazaga said that was just a matter of getting used to this, since the taxa under discussion are of limited importance. He concluded by saying that setting aside the Principle of Priority here provokes more problems than it solves. Ng said he voted FOR because it seemed the best way out of the problem without resorting to a new name or new act. He also noted that this was a ‘cross-disciplinary’ application with both helminthologists and entomologists working together. Bouchet, voting FOR, pointed out that there is a gastropod name *Trigonostoma* Blainville, 1825, which has been made the type of the family group name TRIGONOSTOMINAE Cossmann, 1899 (p. 5). The state of the art is to regard it as a synonym of the subfamily Cancellariinae of the CANCELLARIIDAE Forbes & Hanley, 1851. However, no phylogeny of the CANCELLARIIDAE has been published to date. Molecular work is in progress (Modica & Oliverio, University of Rome), and we do not yet know which names will be needed in the future to classify the genera of CANCELLARIIDAE. He hesitated, though, to add this name to the Case being discussed, because there is currently no need to make a decision on the gastropod family group name, and certainly no need to place it on an Official List with whatever spelling. Gastropod family group names based on a genus with the suffix -stoma (meaning mouth) have historically variously been formed in –STOMIDAE or –STOMATIDAE. In his nomenclator of gastropod families (Bouchet & Rocroi, 2005, *Malacologia*, 47), he and his co-author argued that mnemonics is best served by consistently deriving them in –STOMATIDAE (also in accordance with Appendix D, Table 2, of the 3rd edition of the Code) and their nomenclator entry is under TRIGONOSTOMATINAE. Bouchet said that he expects that, if and when a family-group name of cancellariids based on *Trigonostoma* Blainville, 1825, becomes necessary, TRIGONOSTOMATINAE is the spelling that malacologists should use without the need to submit the case to the Commission.

Original references

The following are the original references to the names placed on Official Lists and Indexes by the ruling given in the present Opinion:

- mucoreum*, *Trigonostomum*, Burmeister, 1844, *Handbuch der Entomologie*, vol. 4(1): 467.
setigerum, *Trigonostomum*, Schmidt, 1852, *Sitzungsberichte der kaiserlichen Akademie der Wissenschaften zu Wien, Mathematisch-Naturwissenschaftliche Classe*, 9: 500.
 TRIGONOSTOMIDAE Graff, 1905, *Zeitschrift für Wissenschaftliche Zoologie*, 83: 109.
 TRIGONOSTOMINA Ohaus, 1912, *Deutsche Entomologische Zeitschrift*, 1912: 151.
Trigonostomum Burmeister, 1844, *Handbuch der Entomologie*, vol. 4(1): 466.
Trigonostomum Schmidt, 1852, *Sitzungsberichte der kaiserlichen Akademie der Wissenschaften zu Wien, Mathematisch-Naturwissenschaftliche Classe*, 9: 500.
Trigonostomus Brenske, 1893, *Societas Entomologica*, 8: 1.

OPINION 2224 (Case 3371)**ARANEIDAE Clerck, 1758, *Araneus* Clerck, 1758 and *Tegenaria* Latreille, 1804 (Arachnida, Araneae): proposed conservation**

Abstract. The Commission has confirmed the status of the family-group name ARANEIDAE (originally Aranei) with the authorship Clerck, 1758 for a family of spiders, designated *Araneus angulatus* Clerck, 1758 as the type species for the generic name *Araneus* Clerck, 1758, and conserved the generic name *Tegenaria* Latreille, 1804 by suppression of its senior objective synonym *Aranea* Linnaeus, 1758.

Keywords. Nomenclature; taxonomy; ARANEIDAE; *Araneus*; *Aranea*; *Tegenaria*; *Araneus angulatus*; spiders.

Ruling

- (1) Under the plenary power it is hereby ruled that:
 - (a) the status of a family-group name for ARANEIDAE Clerck, 1758 (original spelling Aranei), with the type genus *Araneus* Clerck, 1758, is confirmed;
 - (b) all fixations of type species for *Araneus* Clerck, 1758 before that by Simon (1895) are set aside and *Araneus angulatus* Clerck, 1758 is designated as the type species of the generic name *Araneus* Clerck, 1758, as proposed by Simon (1895);
 - (c) the name *Aranea* Linnaeus, 1758 is suppressed for the purposes of the Principle of Priority but not for those of the Principle of Homonymy.
- (2) The name *Araneus* Clerck, 1758 (gender: masculine), type species by subsequent designation by Simon (1895) *Araneus angulatus* Clerck, 1758, as ruled in (1)(b) above, is hereby placed on the Official List of Generic Names in Zoology.
- (3) The name ARANEIDAE Clerck, 1758, type genus *Araneus* Clerck, 1758, is hereby placed on the Official List of Family-Group names in Zoology.
- (4) The name *Aranea* Linnaeus, 1758, as suppressed in (1)(c) above, is hereby placed on the Official Index of Rejected and Invalid Generic Names in Zoology.

History of Case 3371

An application to: (1) confirm the status of the family-group name ARANEIDAE (originally Aranei) with the authorship Clerck, 1758 for a family of spiders; (2) designate *Araneus angulatus* Clerck, 1758 as the type species for the generic name *Araneus* Clerck, 1758; (3) conserve the generic name *Tegenaria* Latreille, 1804 by suppression of its senior objective synonym *Aranea* Linnaeus, 1758 was received from Nikita J. Kluge (*St. Petersburg State University, St. Petersburg, Russia*) on 23 November 2005. After correspondence the case was published in BZN 64: 15–18. The title, abstract and keywords of the case were published on the Commission's website. Two supportive comments and one adverse comment were published in BZN 65: 46; an additional comment by the author of the application was published

in BZN 65: 47. After the voting was complete F. Welter-Schultes submitted a comment to the Secretariat noting that Clerck's 'Aranei' appeared in the Latin text as a translation of the Swedish word 'spindel' or 'spindlar'; thus this word should be regarded as a Latin vernacular and not as a scientific name.

Decision of the Commission

On 1 June 2008 the members of the Commission were invited to vote on the proposals published in BZN 64: 17. At the close of the voting period on 1 September 2008 the votes were as follows:

Affirmative votes – 13: Bogutskaya, Bouchet, Brothers, Fautin, Grygier, Halliday, Krell, Kullander, Lamas, Lim, Pape, Papp and Zhang.

Negative votes – 2: Kottelat and van Tol.

Rosenberg abstained.

No vote was received from Alonso-Zarazaga and Ng. Minelli, Patterson, Pyle and Štys were on leave of absence.

Rosenberg, ABSTAINING, said it was not clear why *Aranei* should be considered a family group name rather than the plural of a generic name; Clerck did not use his stated generic names in combination with his species names, but in some places used *Araneus* instead. He suggested it may be better to rule that *Aranea* Linnaeus was an emendation of *Araneus* Clerck.

Original references

The following are the original references to the names placed on Official Lists and Indexes by the ruling given in the present Opinion:

Aranea Linnaeus, 1758, *Systema Naturae*, Ed. 10, vol. 1. Salvii, Holmiae, p. 619.

ARANEIDAE Clerck, 1758, *Svenska spindlar . . . Aranei Svecici, descriptionibus et figuris . . . illustrati*. vii, Literis Laur. Salvii, Stockholmiae.

Araneus Clerck, 1758, *Svenska spindlar. . . Aranei Svecici, descriptionibus et figuris . . . illustrate*. vii, Literis Laur. Salvii, Stockholmiae, p. 22.

The following is the reference for the designation of *Araneus angulatus* Clerck, 1758 as the type species of the nominal genus *Araneus* Clerck, 1758:

Simon, E. 1892–1903. *Histoire naturelle des araignées*, p. 829.

OPINION 2225 (Case 3412)***Reticulitermes* Holmgren, 1913 (Insecta, Isoptera) given precedence over *Maresa* Giebel, 1856**

Abstract. The generic name *Reticulitermes* Holmgren, 1913, for an economically important and universally known group of subterranean termites responsible for most economic damage in the Northern Hemisphere, was conserved by being given precedence over *Maresa* Giebel, 1856.

Keywords. Nomenclature; taxonomy; Isoptera; *Reticulitermes*; *Maresa*; *Hemerobites*; *Reticulitermes flavipes*; *Maresa plebeja*; termites; fossil; Baltic amber; Eocene.

Ruling

- (1) Under the plenary power it is hereby ruled that the genus-group name *Reticulitermes* Holmgren, 1913 is to be given precedence over *Maresa* Giebel, 1856 whenever their type species are placed in the same genus-group taxon.
- (2) The following names are placed on the Official List of Generic Names in Zoology:
 - (a) *Reticulitermes* Holmgren, 1913 (gender: masculine), type species by original designation *Termes flavipes* Kollar, 1837, with the endorsement that it is to be given priority over the name *Maresa* Giebel, 1856 whenever their type species are placed in the same genus-group taxon;
 - (b) *Maresa* Giebel, 1856 (gender: feminine), type species by monotypy *Maresa plebeja* Giebel, 1856, with the endorsement that it is not to be given priority over the name *Reticulitermes* Holmgren, 1913 whenever their type species are placed in the same genus-group taxon.
- (3) The following names are placed on the Official List of Specific Names in Zoology:
 - (a) *flavipes* Kollar, 1837, as published in the binomen *Termes flavipes* (specific name of the type species of *Reticulitermes* Holmgren, 1913);
 - (b) *plebeja* Giebel, 1856, as published in the binomen *Maresa plebeja* (specific name of the type species of *Maresa* Giebel, 1856).

History of Case 3412

An application to conserve the usage of the generic name *Reticulitermes* Holmgren, 1913 was received from M.S. Engel (*University of Kansas, Lawrence, KS, U.S.A.*) and K. Krishna (*American Museum of Natural History, New York, NY, U.S.A.*) on 6 February 2007. After correspondence the case was published in BZN 64: 230–234. The title, abstract and keywords of the case were published on the Commission's website. One supportive comment was published in BZN 65: 136.

Decision of the Commission

On 1 September 2008 the members of the Commission were invited to vote on the proposals published in BZN 64: 231–232. At the close of the voting period on 1 December 2008 the votes were as follows:

Affirmative votes – 18: Bogutskaya, Bouchet, Brothers, Fautin, Grygier, Halliday, Kottelat, Krell, Kullander, Lamas, Minelli, Pape, Papp, Patterson, Rosenberg, Štys, van Tol and Zhang.

Negative votes – 2: Alonso-Zarazaga and Lim.

No vote was received from Ng. Pyle was on leave of absence.

Alonso-Zarazaga, voting AGAINST, commented that although he agreed with the general purpose of this Case, several points were still obscure. The reference presented as Anonymous (1833) was attributed to Kollar by Sherborn, it being based on a previous apparent nomen nudum, *Termes floripes* Kollar, 1832 (*Amtlicher Bericht über die Versammlung Deutscher Naturforscher und Aerzte*, 1832, p. 100) that the authors did not mention. Bouchet, voting FOR, said he profoundly disliked these ‘conditional reversals of precedence’ (which, in his view, could lead to nomenclatural instability) but said that the voting paper provided no other choice to conserve *Reticulitermes* as it should. He therefore voted for the conservation of *Reticulitermes*, but disapproved of the technical mechanism employed to achieve this goal. Pape, voting FOR, addressed the question asked by other Commissioners ‘Has *Maresa* been formally synonymised with *Reticulitermes*?’ He said yes, it had, although apparently indirectly. Under Case paragraph 1) it was noted that *Hemerobites antiquus* was transferred to *Reticulitermes* Holmgren by Rosen (1913), and under 4) it was mentioned that ‘*Maresa plebeja* was recognised as a synonym of *Hemerobites antiquus* by Hagen (1858, p. 180)’. From this it followed that *Maresa* (monotypic) is a subjective synonym of *Reticulitermes*. Thus he endorsed protecting *Reticulitermes*.

Original references

The following are the original references to the names placed on Official Lists by the ruling given in the present Opinion:

- flavipes*, *Termes*, Kollar, 1837, *Naturgeschichte der schädlichen Insekten in Beziehung auf Landwirthschaft und Forstcultur*, & c. viii, Ferdinand Ullrich, Wien, p. 411.
- Maresa* Giebel, 1856, *Fauna der Vorwelt* [vol. 2]: *Die Insekten und Spinnen der Vorwelt mit Steter Berücksichtigung der Lebenden Insekten und Spinnen*. xviii, F.U. Brockhaus, Leipzig, p. 298.
- plebeja*, *Maresa*, Giebel, 1856, *Fauna der Vorwelt* [vol. 2]: *Die Insekten und Spinnen der Vorwelt mit Steter Berücksichtigung der Lebenden Insekten und Spinnen*. xviii, F.U. Brockhaus, Leipzig, p. 298.
- Reticulitermes* Holmgren, 1913, *Kungliga Svenska Vetenskaps-Akademiens Handlingar*, **50**(2): 60–61.

OPINION 2226 (Case 3359)***Germarostes* Paulian, 1982 and *Haroldostes* Paulian, 1982 (Insecta, Coleoptera, SCARABAEIDAE): generic names not conserved by suppression of a senior synonym**

Abstract. A proposal to conserve the generic name *Germarostes* Paulian, 1982 and the subgeneric name *Haroldostes* Paulian, 1982 for a group of New World scarab beetles by suppression of the senior subjective synonym *Sphaerelytrus* Blanchard, 1841 was not supported by the Commission.

Keywords. Nomenclature; taxonomy; SCARABAEIDAE; CERATOCANTHINAE; *Sphaerelytrus*; *Germarostes*; *Haroldostes*; New World; scarab beetles.

Ruling

- (1) A proposal for conservation of the generic name *Germarostes* Paulian, 1982 and the subgeneric name *Haroldostes* Paulian, 1982 by suppression of the senior subjective synonym *Sphaerelytrus* Blanchard, 1841 was not approved.
- (2) No names are placed on Official Lists or Indexes in this ruling.

History of Case 3359

An application to conserve the generic name *Germarostes* Paulian, 1982 and the subgeneric name *Haroldostes* Paulian, 1982 for a group of New World scarab beetles by suppression of the senior subjective synonym *Sphaerelytrus* Blanchard, 1841 was received from H.F. Howden (*Canadian Museum of Nature, Ottawa, ON, Canada*) on 29 August 2005. After correspondence the case was published in BZN 63: 239–242. The title, abstract and keywords of the case were published on the Commission's website. No comments on this case were received.

Decision of the Commission

On 1 September 2007 the members of the Commission were invited to vote on the proposals published in BZN 63: 240. At the close of the voting period on 1 December 2007 a majority of Commissioners voted FOR the Case (14 FOR, 8 AGAINST) but this failed to reach the two-thirds majority required for approval. No Commissioner's comments were received in this round of voting.

On 1 March 2008 the members of the Commission were again asked to vote on the proposals in BZN 63: 240 in accordance with Bylaw 35. At the close of the voting period on 1 June 2008 the votes were as follows:

Affirmative votes – 8: Bouchet, Halliday, Krell, Kullander, Mawatari, Papp, Štys and Zhang.

Negative votes – 8: Brothers, Fautin, Grygier, Kottelat, Lamas, Pape, Rosenberg and van Tol.

Bogutskaya abstained. No vote was received from Lim. Alonso-Zarazaga, Minelli, Ng, Patterson and Pyle were on leave of absence.

Bogutskaya, ABSTAINING, said she saw inconsistency between the statement that 'Although *Sphaerelytrus* has not been used since 1899, Article 23.9.1 cannot be

used for an automatic reversal of precedence because the names *Haroldostes* and *Germarostes* have been used in fewer than 25 publications' and the number of publications given for *Germarostes* since 1982, which was exactly 25. In addition, all other requirements of Article 23.9.1 were met (i.e. use by more than ten authors over ten years) for the name *Germarostes* to have precedence over *Sphaerelytrus* without special ruling of the Commission. Bouchet, voting FOR, noted that the name *Haroldostes* appears to have been used only seven times since it was established and he would in principle favour strict application of priority of *Sphaerelytrus*. However, and despite the fact that the usages of *Germarostes* Paulian, 1982 and *Sphaerelytrus* Blanchard, 1841 satisfy Article 23.9 of the Code, the legitimate restored usage of *Sphaerelytrus* at genus level in place of *Haroldostes* could then lead to displacement of *Germarostes* by all authors who consider *Haroldostes* as a synonym or subgenus of *Sphaerelytrus*. Bouchet pointed out that this would be a cause of instability, and therefore he voted for the suppression of *Sphaerelytrus*. Brothers, voting AGAINST, said that it appeared that plenary action was unnecessary in the case of *Germarostes* and that it qualified for reversal of precedence with respect to *Sphaerelytrus*. He felt no convincing case had been made on grounds of extensive usage outside of taxonomy for protection of *Haroldostes*.

No names are placed on Official Lists or Indexes in this ruling and the issue is left open for subsequent workers to follow the precepts of the Code.

OPINION 2227 (Case 3375)***Fidia* Baly, 1863 and *Lypesthes* Baly, 1863 (Insecta, Coleoptera):
usage not conserved and priority maintained for *Fidia* Motschulsky,
1860**

Abstract. The Commission has ruled that priority is maintained for the leaf beetle generic name *Fidia* Motschulsky, 1860, thus it remains the senior homonym of *Fidia* Baly, 1863 and senior objective synonym of *Lypesthes* Baly, 1863.

Keywords. Nomenclature; taxonomy; Coleoptera; CHRYSOMELIDAE; EUMOLPINAE; *Fidia*; *Lypesthes*; *Lypesthes atra*; *Fidia viticida*; leaf beetles.

Ruling

- (1) It is hereby ruled that the following names are not conserved:
 - (a) *Fidia* Baly, 1863;
 - (b) *Lypesthes* Baly, 1863;
 - (c) *viticida* Walsh, 1867, as published in the binomen *Fidia viticida*.
- (2) No names are placed on Official Lists or Indexes in this ruling.

History of Case 3375

An application to conserve the usage of the generic names *Fidia* Baly, 1863 and *Lypesthes* Baly, 1863 for leaf beetle genera by suppressing the name *Fidia* Motschulsky, 1860 (senior homonym of *Fidia* Baly, 1863 and senior objective synonym of *Lypesthes* Baly, 1863) was received from M.S. Strother and V.M. Bayless (Louisiana State Arthropod Museum, Baton Rouge, LA, U.S.A.) and C.L. Staines (National Museum of Natural History, Smithsonian Institution, Washington, DC, U.S.A.) on 21 November 2006. After correspondence the case was published in BZN 64: 35–38. The title, abstract and keywords of the case were published on the Commission's website. No comments on this case were received.

Decision of the Commission

The Case was sent to the members of the Commission for voting on 1 December 2007. The case received a majority of the votes cast (11 FOR, 7 AGAINST), but failed to reach the two-thirds majority required for approval. Brothers, voting FOR, commented that it seemed illogical to include an invalid name as the name of a type species without any qualification. Kottelat, voting AGAINST, said that the application did not provide information on why the use of *lurida* (the suppression of which for the purposes of the Principle of Priority had been requested) would create instability and what would be the consequences of not suppressing *Fidia* Motschulsky and *lurida*. He did not want to suppress *lurida* without knowing why and what he was suppressing, adding further that *Fidia* and *lurida* are two independent cases. Štys, voting AGAINST, said that he could agree with authors' requests (1a), (2a), (2b), (3b) and 4, but not with (1b), (3a), (5) and therefore he voted AGAINST the whole proposal, and advocated that it should be reconsidered and subjected to a new vote.

He added that he felt that the authors of the application did not provide appropriate evidence that *Fidia lurida* Baly, 1863 is a junior synonym of *Fidia viticida* Walsh, 1867, and that the mere reference to several catalogues and general works on the CHRYSOMELIDAE and/or fauna of North America is not sufficient evidence. He felt the Commission was being asked to decide upon a taxonomic problem by nomenclatural means without being sufficiently informed. Bouchet, voting AGAINST, said that the application did not adequately document the prevailing usage of *Fidia* Baly, 1863 (mentioning only three references in the last 50 years, 'and others') and *Lypestes* Baly, 1863 (mentioning only six references by four authors in the last 50 years, 'and others'). He said that the application did not refer to usage of these names outside a very small segment of the taxonomic literature and thus he saw no compelling reason to use the plenary power to suppress the name *Fidia* Motschulsky, 1860. Grygier, voting AGAINST, stated that the matters concerning these two genera are independent and should have been voted on separately, and, additionally, that the species matter (*lurida/viticida*) was independent. Given the option, he would have voted in favour of conserving the two genus names, but the species-level problem was brought in as an afterthought with no detailed explanation; it is specifically against this that he objected. He pointed out that with the Voting Papers worded as they were, all these matters had to be subsumed under a single vote, and particularly owing to the insufficient presentation of the species-level proposal, his vote was negative. Ng, voting AGAINST, said there were several points that needed clarification. One was whether the names were as widely used as indicated – certainly the number of papers indicated in the Case were not extensive enough to merit plenary intervention. He said that the change of an author's name and year was not significant and was easily appreciated by any taxonomist. He added that there is no basis for the names for these taxa being broadly used or of economic or commercial significance.

In accordance with Bylaw 35 the Case was sent for a revote on 1 December 2008. At the close of the voting period on 1 March 2009 the votes were as follows:

Affirmative votes – 4: Fautin, Papp, van Tol and Zhang.

Negative votes – 14: Alonso-Zarazaga, Bogutskaya, Bouchet, Brothers, Halliday, Kottelat, Kullander, Lamas, Lim, Minelli, Ng, Pape, Rosenberg and Štys.

Split votes: Grygier voted FOR proposals 1(a), 2, 3(b), 4, 5 and AGAINST: 1(b), 3(a); Krell voted FOR 1(a), 2(a), 2(b), 3(b), 4; AGAINST 1(b), 3(a), 5. Thus total votes for each proposal were as follows:

1 (a): 6 FOR; 14 AGAINST.

1 (b): 4 FOR; 16 AGAINST.

2 (a), (b): 6 FOR; 14 AGAINST.

3 (a): 4 FOR; 16 AGAINST.

3 (b): 6 FOR; 14 AGAINST.

4: 6 FOR; 14 AGAINST.

5: 5 FOR; 15 AGAINST.

Patterson and Pyle were on leave of absence.

Alonso-Zarazaga, voting AGAINST, said he could not see the need for disturbing the Principle of Priority in this particular case, where the authors did not succeed in demonstrating any real harm to nomenclature if the original uses of the generic and specific names are maintained. Brothers, voting AGAINST, explained that upon reconsideration he found the case unconvincing, especially since no detailed evidence

was provided of the actual consequences of not acting, and the names of the taxa involved apparently have no significance outside taxonomy. Grygier said that he felt he could split his vote this time, which was his preference in the first round of voting, as it was explicitly allowed for in the second voting paper. Halliday said that although he voted FOR this Case last time, he was voting AGAINST it in the second round as he felt on reflection that the Case did not provide enough information. He felt there was no evidence that *viticida* and *lurida* are synonyms nor enough evidence of the degree of usage of the names *Fidia* and *Lypesthes*. Halliday also said that the generic and specific questions were separate; whether to use *Fidia* or *Lypesthes* was separate from whether to use *lurida* or *viticida*.

No names are placed on Official Lists or Indexes and the issue is left open for subsequent workers to follow the precepts of the Code or to make new proposals to the Commission.

OPINION 2228 (Case 3393)***Dactylozodes* Chevrolat, 1838 (Insecta, Coleoptera): name not conserved**

Abstract. A proposal to conserve the generic name *Dactylozodes* Chevrolat, 1838 for a buprestid (jewel beetle) by suppression of its senior subjective synonym *Lasionota* Mannerheim, 1837 was not accepted.

Keywords. Nomenclature; taxonomy; Coleoptera; BUPRESTIDAE; *Dactylozodes*; *Lasionota*; South America; jewel beetles.

Ruling

- (1) It is hereby ruled that the proposal to conserve the generic name *Dactylozodes* Chevrolat, 1838 by suppression of the generic name *Lasionota* Mannerheim, 1837 is not accepted.
- (2) No names are placed on Official Lists or Indexes in this ruling.

History of Case 3393

An application was received from C.L. Bellamy (*Plant Pest Diagnostics Branch, California Department of Food & Agriculture, Sacramento, CA, U.S.A.*) and T.M. Rodriguez (*Santiago, Chile*) on 27 July 2006. After correspondence the case was published in BZN 64: 43–44 (March 2007). The title, abstract and keywords of the case were published on the Commission's website. One supportive comment and one comment with author's amendment to the application were published in BZN 64(2): 124.

Decision of the Commission

On 1 December 2007 the members of the Commission were invited to vote on the proposals published in BZN 64: 44. At the close of the voting period on 1 March 2008 a majority of Commissioners voted FOR the Case (11 For, 9 Against), but this failed to reach a two-thirds majority required for approval. In accordance with the Bylaws it was sent for a revote on 1 September 2008. At the close of the voting period on 1 December 2008 the votes were as follows:

Affirmative votes – 5: Krell, Lim, Patterson, Štys and Zhang.

Negative votes – 15: Alonso-Zarazaga, Bogutskaya, Bouchet, Brothers, Fautin, Grygier, Halliday, Kottelat, Kullander, Lamas, Minelli, Pape, Papp, Rosenberg and van Tol.

No vote was received from Ng. Pyle was on leave of absence.

Alonso-Zarazaga, voting AGAINST, said that this case should have been presented as a reversal of precedence under Article 23.9.3, since it was very likely that the requirements of Article 23.9.1.2 were not met, *Dactylozodes* not being a genus of importance except for a few specialists. The authors did not state whether the type species of *Lasionota*, namely *L. quadrifasciata*, was currently deemed to be a member of *Dactylozodes*. The authors failed to evaluate the impact on lepidopteran nomenclature of *Lasionota* Mannerheim being suppressed, since the presently invalid

Lasionota Warren, 1912 would then be valid again. Its replacement name *Isatoolna* Nye, 1975 seemed to have been in common use since its proposal, and the authors had not evaluated the consequences of changing back to *Lasionota* Warren. Bouchet, voting AGAINST, pointed out that *Lasionota* Mannerheim, 1837 could qualify as a nomen oblitum, but the application gave references to only three authors that had used *Dactylozodes* in the last 50 years: Gardner (1989), Moore (1997) and the author of the application Bellamy (1998, 2003, 2006). This was not sufficient, in his view, to protect *Dactylozodes*, and thus he voted in favour of Priority. Halliday said he voted AGAINST the proposal for two reasons. First, no evidence was presented to show whether or not *Lasionota* and *Dactylozodes* were synonymous, except for Lacordaire (1857). The most we could do under these circumstances was to rule that *Dactylozodes* had precedence over *Lasionota* when these names were considered as synonyms, not unconditional suppression of *Lasionota*. Second, the evidence for prevailing usage of *Dactylozodes* was very thin. Apart from papers written by the authors of the present proposal, there were only a handful of usages of *Dactylozodes*. Adherence to the Principle of Priority would not result in any disruption to nomenclature. Pape, voting AGAINST, commented that this was a case relating to the ‘fuzzy’ concept of prevailing usage. He said that the Code had very explicit requirements for reversal of precedence, and when these were not fulfilled there should be very good reasons, i.e. something that went beyond prevailing usage, for a ruling. He did not feel these were presented in the Case and so voted against. Similarly, Rosenberg voted AGAINST with the comment that, according to the application, *Lasionota* had not be used as valid since 1857, but only seven uses of *Dactylozodes* since 1899 were cited, none of them outside the field of systematic entomology. He felt that there was no compelling reason not to follow priority in this case. He pointed out that even if the application was declined by the Commission, if *Dactylozodes* was more widely used than the application suggested, the author could give precedence to *Dactylozodes* under Article 23.9, if enough uses of the name as valid could be found.

No names are placed on Official Lists or Indexes in this ruling and the issue is left open for subsequent workers to follow the precepts of the Code or to make new proposals to the Commission.

Corrigendum to Opinion 2218 (Case 3403)

***Mecistocephalus* Newport, 1843 and *Pachymerium* Koch, 1847 (Chilopoda): current usage conserved by designation of *Mecistocephalus punctifrons* Newport, 1843 as the type species of *Mecistocephalus* Newport, 1843 (BZN 66: 93–94)**

Ruling (2)(a) in Opinion 2218 is to be read as follows:

- (b) *Pachymerium* Koch, 1847 (gender: neuter), type species *Geophilus ferrugineus* Koch, 1835, by monotypy.

Erratum

The correct number for the Opinion on Case 3405 *Trigonostomum* Schmidt, 1852 (Platyhelminthes, TRIGONOSTOMIDAE) and *Trigonostomus* Brenske, 1893 (Coleoptera, SCARABAEIDAE): generic names conserved is 2229.

This has been corrected on the hardcopy and on the PDF before distribution.

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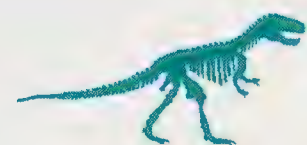
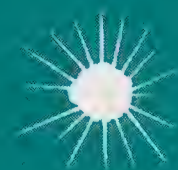
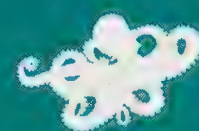
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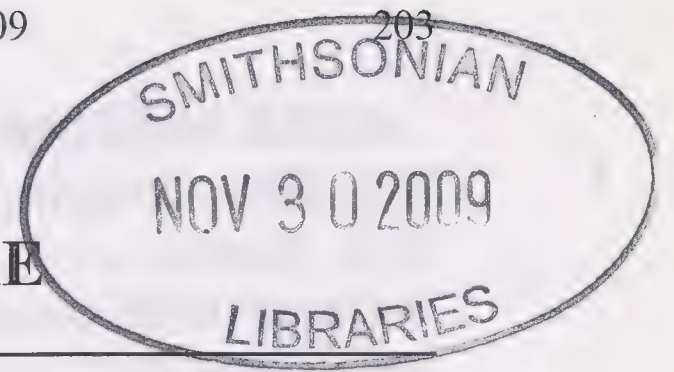
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BULLETIN OF ZOOLOGICAL NOMENCLATURE

Volume 66, part 3 (pp. 203–296)

30 September 2009

Notices

(1) Applications and correspondence relating to applications to the Commission should be sent to the Executive Secretary at the address given on the inside of the front cover and on the Commission website. English is the official language of the *Bulletin*. Please take careful note of instructions to authors (present in a one or two page form in each volume and available online at <http://www.iczn.org/guidelines.html>) as incorrectly formatted applications will be returned to authors for revision. The Commission's Secretariat will answer general nomenclatural (as opposed to purely taxonomic) enquiries and assist with the formulation of applications and, as far as it can, check the main nomenclatural references in applications. Correspondence should be sent by e-mail to 'iczn@nhm.ac.uk' where possible.

(2) The Commission votes on applications eight months after they have been published, although this period is normally extended to enable comments to be submitted. Comments for publication relating to applications (either in support or against, or offering alternative solutions) should be submitted as soon as possible. Comments may be edited (see instructions for submission of comments at http://www.iczn.org/Instructions_for_comments.html).

(3) Requests for help and advice on the Code can be made direct to the Commission and other interested parties via the Internet. Membership of the Commission's Discussion List is free of charge. You can subscribe and find out more about the list at <http://list.afriherp.org/mailman/listinfo/iczn-list>.

(4) The Commission also welcomes the submission of general-interest articles on nomenclatural themes or nomenclatural notes on particular issues. These may deal with taxonomy, but should be mainly nomenclatural in content. Articles and notes should be sent to the Executive Secretary.

New applications to the Commission

The following new applications have been received since the last issue of the *Bulletin* (volume 66, part 2, 30 June 2009) went to press. Under Article 82 of the Code, the existing usage of names in the applications is to be maintained until the Commission's rulings on the applications (the Opinions) have been published.

CASE 3496: *Massospondylus carinatus* Owen, 1854 (Dinosauria, Sauropodomorpha): proposed conservation of usage by designation of a neotype. A.M. Yates & P.M. Barrett.

CASE 3497: *Cyphon palustris* Thomson, 1855 (Insecta, Coleoptera): proposed conservation of the specific name. O. Vorst.

CASE 3498: *Eupales* Lefèvre 1885 and EUPALINI Verma, Gomez-Zurita, Jolivet & Vig, 2005 (Insecta, Coleoptera, EUMOLPINAE): proposed conservation. P. Jolivet & K.K. Verma.

CASE 3499: *Creadion* Vieillot, 1816 (Aves): proposed suppression. W.J. Bock & R. Schodde.

CASE 3500: PARADISAEIDAE and *Paradisaea* Linnaeus, 1758 (Aves): proposed conservation of usage. R. Schodde & M. LeCroy.

CASE 3501: *Tyrophagus putrescentiae* (Schrank, 1781) (Acariformes, ACARIDAE): proposed conservation of usage by designation of a replacement neotype. P.B. Klimov & B.M. OConnor.

CASE 3502: *Coluber nummifer* Reuss, 1834 (currently *Hemorrhois nummifer*; Reptilia, Serpentes): proposed conservation of the specific name. B. Schätti.

CASE 3503: *Papilio hesperus* Westwood, 1843 (currently *Papilio Hesperus*; Insecta, Lepidoptera, PAPILIONIDAE) and *Papilio hesperus* Fabricius, 1793 (NYMPHALIDAE): proposed conservation of prevailing usage by the suppression of *Papilio hesperus* Fabricius, 1793

Contributions to the Discussion on Electronic Publication II

Introduction

This is the second instalment of comments on the ICZN proposed amendment on electronic-only publication. If the proposed amendment passes review from the IUBS and then a vote from the Commission, it will allow publication of nomenclatural acts on exclusively electronic media to be valid and available. The proposed amendment is available in the BZN **65**: 265–275, several other sources, and online at http://www.iczn.org/electronic_publication.html. We are eager for input from all stakeholders in this process, including taxonomists, publishers, archivists, database experts and the wide range of users of nomenclatural information. Before the Commission's vote there will be one more opportunity for input through the BZN in our subsequent issue and we encourage continued debate through listservers (e.g. ICZN listserver (<http://list.afriherp.org/mailman/listinfo/iczn-list>) and Taxacom (<http://mailman.nhm.ku.edu/mailman/listinfo/taxacom>)) and the various journals that have published the proposed amendment.

Ellinor Michel
Svetlana Nikolaeva
Natalie Dale-Skey
Steve Tracey

Electronic publication of nomenclatural acts is inevitable, and will be accepted by the taxonomic community with or without the endorsement of the Code

Michael P. Taylor

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London, WC1E 6BT, U.K. (e-mail: dino@miketaylor.org.uk)*

Abstract. The recent description of the fossil primate *Darwinius masillae* in the online journal *PLoS ONE* exemplifies an increasingly common problem: nomenclatural acts in non-print venues that are not considered 'published' under the Code's Articles 8.6 and 9.8. Although the name *Darwinius* was subsequently validated by the publication of hardcopy offprints of the electronic paper, other zoological names have been published electronically in this and other online journals, and the broader taxonomic community's acceptance of these invalidly published names suggests that it is the Code itself that is outdated in refusing to recognise names accepted by everyone else. If the Code is not quickly changed to accommodate electronic publication, it will become marginalised and ignored, to the detriment of sense and stability in nomenclature. The increasing prevalence of electronic publishing leaves only a small window of opportunity in which the Code can act to regulate nomenclatural acts in these venues. Fears regarding the conservability of electronic documents are not justified, as the ability to quickly and cheaply make abundant perfect copies makes an electronically published paper impossible to eradicate. Likewise, worries about the

unsuitability of the ubiquitous PDF format for long-term conservation are largely groundless now that the PDF archival format, PDF/A, is an international standard. The world has changed, and in order to remain relevant the Code must serve the world as it actually is, not as we may wish it was.

Keywords. Nomenclature; taxonomy; publishing; electronic publishing; nomenclatural acts; *Darwinius masillae*; PLoS; PDF.

Background: the availability of the name *Darwinius masillae*

The description of the basal primate *Darwinius masillae* on 19 May 2009 (Franzen et al., 2009) generated a great deal of publicity and controversy. Leaving aside issues of the new taxon's phylogenetic affinities, its brief nomenclatural history highlights an important trend. The initial publication was in the online-only journal *PLoS ONE*, a journal of the Public Library of Science (<http://plos.org/>): therefore, as pointed out by various people and summarised by Zimmer (2009a) the day after publication, the name *Darwinius masillae* is not available under Article 8.6 of the Code. Article 9.8 explicitly states that 'none of the following constitutes published work within the meaning of the Code: [...] text or illustrations distributed by means of electronic signals (e.g. by means of the World Wide Web)'.

After consultation between the journal, the Commission and the Secretariat (described in Zimmer, 2009b), a way forward was found: within one further day, the situation was remedied by the publication of fifty printed copies of the paper, which were made available for a nominal fee of \$10 by mail order. These printed copies are identical to the original publication apart from the addition of a cover sheet stating that 'This document was produced by a method that assures numerous identical & durable copies, and those copies were simultaneously obtainable for the purpose of providing a public and permanent scientific record, in accordance with Article 8.1 of the International Code of Zoological Nomenclature. Date of publication: 21st May 2009' (Zimmer, 2009c). Thus the name *Darwinius masillae* was validly published for nomenclatural purposes two days after initial publication. It is not clear whether or how the two publications, electronic and printed, can be cited unambiguously, but at least now the Code is satisfied and the name is safe from nomenclatural claim-jumping. We will refer to this approach of publishing hardcopy offprints after an initial electronic-only publication as 'the *Darwinius* solution'.

In the wake of the *Darwinius* debacle, lessons have been learned: a more recent paper in *PLoS ONE* (Hocknull et al., 2009) named three new monospecific dinosaur genera: *Wintonotitan*, *Diamantinasaurus* and *Australovenator*, and that paper contained a statement that printed copies of the paper were made available, simultaneously with electronic publication, in order to satisfy Article 8.1 of the Code. This is more satisfactory than the after-the-event repairs enacted to save *Darwinius*, but two issues remain.

First, the *Darwinius* case, while high-profile, is not unique: other new taxa recently named in *PLoS ONE* include the theropod dinosaur *Aerosteon* (Sereno et al., 2008), the primitive whale *Maiacetus* (Gingerich et al., 2009) and the ancestral sauropodomorph dinosaur *Panphagia* (Martinez & Alcober, 2009), none of which

names was available under the Code. (Following *Darwinius*, these names have since been validated by the subsequent production of offprints.) The PLoS journals are not alone in publishing new names electronically: for example, in *Proceedings of the Royal Society B*, the stegosaurid dinosaur *Miragaia* was published online on 25 February 2009 (Mateus et al., 2009a), nearly three months before the printed version followed on 22 May (Mateus et al., 2009b). Other recent names published electronically in *Proceedings B* before printed publication include the basal sauropod dinosaur *Antetonitrus* (Yates & Kitching, 2003), the basal suchian *Effigia* (Nesbitt & Norell, 2006), the burrowing ornithopod dinosaur *Oryctodromeus* (Varricchio et al., 2007), the basal ornithischian dinosaur *Eocursor* (Butler et al., 2007) and the theropod dinosaur *Austroraptor* (Novas et al., 2008). Electronic publishing of new names has arrived.

Second, is the Code, as currently established, serving nomenclature? Or is nomenclature serving the Code? While the case of *Darwinius* shows that band-aid solutions can be applied to solve some of the problems of electronic publishing, such solutions arguably put the cart before the horse by requiring legalistic adherence to rules that a changing world has rendered obsolete. Paul van Rijkevorsel (2009), in a message on the ICZN listserver, spoke for many when he expressed distaste for the *Darwinius* solution by writing that 'With print shops in every town that will turn out booklets on demand, cheaply and fast, surely no prestigious journal (online or not) would feel comfortable in making do with a mere stapled set of printed sheets? Impress on them how silly it looks to be remembered for all time by a stapled set of printed sheets'. But the truth is that the journal, the authors and the taxon will not be remembered by the stapled set of printed sheets – they will be remembered by the freely available PDF that every interested zoologist has downloaded, read, added to their repositories, backed up using their various private schemes, and sent to their friends. The reality is that nobody outside of the ICZN and its associated listserver cares about the printed copies – so far as the rest of the world is concerned, they are nothing more than a box-checking exercise.

The Code is in danger of becoming an irrelevance

While the *Darwinius* solution is obviously not ideal, it is not clear that the case of *Miragaia* (and other new taxa published in *Proceedings B*) is much better. Although the journal no doubt intended the online and printed versions of the *Miragaia* paper (Mateus et al., 2009a, b) to be two manifestations of the same work, the fact that only the latter is validly published for nomenclatural purposes means that careful discussion must treat them separately. While the Code insists that the name *Miragaia* did not become valid until May 2009, the vertebrate palaeontology community treated the name as valid from its initial online publication three months earlier. Although technically a three-month window existed during which the new stegosaur was vulnerable to 'retro-scooping' by any worker unethical enough to apply a new name to the specimen in a printed publication, there is very little chance that such a name would have been recognised by the community: the reality, whatever the Code says, is that for most working zoologists, electronic publication is sufficient to establish priority. Even in the case of the three new Australian dinosaurs published in *PLoS ONE* (Hocknull et al., 2009), for which printed copies were made available from the date of electronic publication, the Code's insistence that only the printed

copies that no-one has are 'real', and the globally distributed electronic copies are not, does not serve the community, and is unlikely to be honoured in future citations of the new names.

In light of the mismatch between the rules laid down by the Code and those followed in practice by increasingly many working zoologists, it is far from clear that the Commission has the power to enforce rules perceived as obsolete by the broad and diverse community of zoologists. Regarding the initial unavailability of *Darwinius*, much online discussion ensued: comments such as the following, from Dr Adam Yates of the University of the Witwatersrand, are representative:

'It seems to me that the code is in danger of becoming an irrelevance. Its very existence depends on the community agreeing to respect and adhere to its rules. I strongly suspect that people will simply ignore the restriction on electronic publications and continue to cite and use *Darwinius*, *Panphagia*, etc. [. . .] as valid taxa. And if people use them as valid taxa, well then [. . .], Code or not, they ARE valid taxa.' (Yates in Parker, 2009)

In another comment on the same article, Dr Andrew Farke of the Raymond M. Alf Museum commented:

'I think that among many, the code *will* become an irrelevance on account of issues like this. Even if some opportunist renamed taxa like *Maiacetus* and *Darwinius* and *Panphagia* in a 'valid' format (which would likely mean an obscure journal of limited circulation), I suspect that people would ignore these sorts of papers in favour of the original description. I know that I would!' (Farke in Parker, 2009)

It is important to understand that these opinions, and others like them, are not those of uncredentialed commenters, but of qualified, professional, publishing zoologists.

More disturbing still for the Code is the position adopted by the journals. For example, the cover sheet of the initial online publication of *Miragaia* (Mateus et al., 2009a) stated that 'Advance online articles are citable and establish publication priority', explicitly disclaiming the ICZN rule that only printed publications are significant for establishing priority. Again, it is important to note that *PLoS ONE* and *Proceedings B* are reputable journals run by respected scientists, not low-budget in-house publications or the work of amateurs in basements with inkjet printers. In particular, *Proceedings B* is currently on volume 276, and has been published since 1800 by the oldest learned society in the world – not a body that one would normally expect to leap unthinkingly onto bandwagons. That the Royal Society of London is embracing the electronic publication of nomenclatural acts should give pause to all who consider electronic publication to be a dangerous and transitory fad.

Paper journals are going away

The problem of electronic nomenclature is only going to become more ubiquitous as more journals convert to electronic-only formats. This trend is already observable, and will inevitably accelerate due not only to the cost benefits but also to the additional possibilities offered by electronic formats – high-resolution figures, video, etc. An increasing proportion of nomenclatural acts will therefore not be represented in published form to the satisfaction of the Code, but if current practice is a good guide, will nevertheless be recognised by the community. In a carefully argued blog post, Dr Matt Wedel of Western University of Health Sciences wrote:

‘Most online publications are hampered by having to be identical to the dead-tree versions (no links, no embedded video, no rotating 3D PDF images, etc.). Eventually people will realise that it is counterproductive to keep hobbling the new medium to make it as slow, flat, and inefficient as the old medium. Once one journal takes the hobbles off, others will do the same rather than lose contributors to cutting-edge outlets. A few boutique journals may still produce flattened, gutted versions of the online publications on paper. People still fly biplanes, too. Paper-based journals will never be popular again and their existence will not stop people from doing whatever technology allows them to in the online venues.’ (Wedel, 2009)

And Dr Bora Zivkovic, Online Discussion Expert for PLoS, wrote:

‘At this point in time it makes no difference if the paper exists only online, or if it was printed by a traditional publisher, or if the online publisher printed out 50 copies of the PDF, or if it was printed by a user at home on a personal computer printer. With the printing costs high, more and more journals will be online only and the physical dead-tree paper will become an anachronism pretty soon [...] Thus, the medium – paper vs. Web – is completely irrelevant for the purpose of ranking outlets at this moment in history, and will become increasingly so in the near future as all journals stop printing and move online. [...] I guess ICZN is keeping the taxonomy literature behind the times, insisting on paper. [...] Perhaps *Darwinius* sped up the process at which ICZN will move forward and taxonomy journals will then follow and join the rest of the world?’ (Zivkovic in Taylor, 2009)

As a member of the Palaeontological Association and the Society of Vertebrate Paleontology, I receive printed issues of those societies’ journals, *Palaeontology* and the *Journal of Vertebrate Paleontology* – yet in recent years I have hardly ever referred to them: I flick through each issue when it arrives, then shelve it. Most issues never come down from the shelf again, as the PDFs available from the society web-sites are so much more convenient: portable, searchable, containing extractable images. An informal straw-poll conducted across a representative sample of my colleagues showed a 50-50 split between those who read papers primarily in printed form and those who prefer electronic form. But, significantly, the younger workers – the next decade’s establishment – prefer electronic publications much more strongly than their older colleagues. It is not difficult to sense which way the wind is blowing.

The time to act is now

In light of the inexorable move towards electronic publishing either ahead of printed publication (as in *Proceedings B*) or instead of it (*PLoS ONE*), the question is no longer whether electronic-only publications *should* be recognised for nomenclatural purposes. That issue is settled: they *are* recognised in much of the zoological community, and are making further inroads. The remaining question is: will they be recognised under the governance of a revised Code, or without a code? Even if it’s true, as some have argued, that electronically published works are less conservable than those on paper (concerning which see below), that would not stop zoologists from publishing nomenclatural acts in electronic-only journals; and those acts are, and will continue to be, recognised by everyone except a hard core of increasingly isolated nomenclature specialists. Those of us who care about sense and stability in nomenclature must act on the basis of how the world actually is, not how we wish it was.

Right now, the Code has a window of opportunity in which it can ensure that electronic publication is done under its governance and therefore on its terms. If this opportunity is not taken, then electronic publication of nomenclatural acts will continue anyway, but without a code: the result may be anarchy, e.g. no requirement of permanence of published works, no recognition of the importance of immutability, and no respect for priority. The Code exists to prevent such chaos: but it will not be able to do so if it is widely ignored because of its denial of basic realities.

Electronic documents are different from electronic media

Among those who oppose the recognition of electronic publishing for nomenclatural purposes, the most commonly expressed reason is fear that electronically published works are less able to be conserved than printed works – for example ‘Paper is proven to last hundreds or thousands of years, and electronic media are notoriously ephemeral’ (Beccaloni in Michel et al., 2009). This is an important issue which deserves to be addressed. Several points can be made here.

1. Whether or not electronic documents are less persistent than printed documents, they will continue to be published and will continue to contain nomenclatural acts which the taxonomic community will accept as valid. Any impermanence of electronic documents is simply a problem that we have to solve: disengaging because the problem is hard is not an option if the Code is to remain relevant.

2. The concept of ‘electronic information’ has changed dramatically in the last two decades. Not long ago, electronic information was always embodied on a physical medium (floppy disk, quarter-inch cartridge, CD-ROM, etc.) which was vulnerable to degradation and obsolescence. Now that the Internet is ubiquitous in developed countries, electronic documents have their own existence independent of any particular medium on which they are written. Concerns about persistence must be evaluated in this context. Thus the current Article 8.6 (‘Works produced after 1999 by a method that does not employ printing on paper’), as generally understood, is no longer relevant.

3. It is very cheap, very quick and very easy to make arbitrarily many perfect copies of an electronic publication, and to distribute them anywhere in the world: therefore, persistence of electronic publications may be sought not only in carefully preserving a few copies in well-known places, but also in encouraging proliferation of copies. Consider a paper that is conserved by placing copies in six large, well funded archives, each with only a 1% chance of failing; another paper distributed to careless individuals who each have a 50% chance of losing their copies requires only forty such individuals to have a better overall chance of survival ($0.5^{40} < 0.01^6$).

4. Electronic publications that are freely available and unencumbered by copyright restrictions (‘open access’) routinely proliferate from computer to computer and so are effectively archived in hundreds or thousands of locations around the world. To pick a topical example, the *Darwinius* paper now exists in many tens of thousands of identical electronic copies. With or without LOCKSS, Portico and other such systems, there is no chance whatsoever of that publication becoming impossible to track down in the future.

5. Electronic publications that are *not* freely available proliferate anyway, despite the publishers’ wishes, by various clandestine means (email attachments, bulletin boards, USB drives, etc.). A paper, once published on the Internet, is a genie that’s

been let out of the bottle: it cannot be prevented from replicating even by a publisher that would like to suppress it; far less can it be lost inadvertently. (Music publishers are finally accepting this in respect of MP3s of popular songs, years after everyone else realised; academic publishers are learning the same lesson now, although some remain in denial).

6. Persistence of electronic publications is best and most cheaply achieved by allowing and encouraging copying between individuals rather than by maintaining complex, expensive official archives. (This is not to say that official archives have no role; but they are not necessary for a publication to live forever.)

7. Given a printed publication, it is difficult and time-consuming to create an electronic copy by scanning; conversely, given an electronic publication, it is easy and quick to create a hard copy by printing. Libraries are at liberty to print electronic publications on archival paper and conserve the printed copies; proliferation of electronic copies will make this easy to do where artificial copyright barriers do not impede librarians from taking this approach.

We must come to terms with the ubiquity of PDF

Some on the ICZN listserver have argued that while electronic publication would be acceptable in an appropriate format, the currently ubiquitous PDF format is not suitable for preservation because of its supposedly obscure specifications, and its perceived dependence on a single commercial vendor. Instead, an XML-based format is often advocated as a better choice. The problem is inertia: the utter ubiquity of the PDF format in contemporary electronic publishing renders any proposal to deprecate it moot. In light of journals' existing investment in PDF-based publishing pipelines, trying to enforce the use of a 'better' XML-based format, while a noble aspiration, would be a doomed strategy – like trying to replace QWERTY keyboards with more ergonomic alternatives. It simply will not happen. PDFs will continue to be used, whether we like it or not; so solutions must be found to whatever problems beset PDFs.

As it happens, these problems are nowhere near as severe as sometimes portrayed. Criticisms of PDF fall into three main areas, all of them easily addressed:

PDF is often described as a proprietary format, the use of which is dependent on the goodwill of Adobe. Although it was originally a closed format, the PDF specifications are now a matter of public record and have been codified as an international standard, ISO 32000–1:2008.

A fear is sometimes expressed that when Adobe stops supporting Acrobat, PDFs will become unreadable. This is incorrect because of the large number of PDF-reading programs written and maintained outside Adobe. For example, installations of the free operating system Ubuntu GNU/Linux come with copies of xpdf, GhostView, ePDFView, Evince and Okular, all of them open-source software. Eleven open-source readers are listed and linked from <http://pdfreaders.org/>. This software exists on literally millions of computers, and is not going to go away.

The PDF format encompasses many variants, so that a PDF that is readable by one program may not be readable by another. This difficulty is ameliorated by PDF/A, a subset of PDF specifically intended for long-term archiving, which is defined by the international standard ISO 19005–1:2005. Some journals' PDFs are

already PDF/A-compliant, and therefore good candidates for long-term archiving; journals currently producing other PDF dialects would not find it onerous to convert to PDF/A.

In summary, most fears regarding the long-term preservation of PDF files are unfounded or outdated. But even if this were not so, it would not change the fact that journals do publish PDFs and will continue to do so for some time yet, and that zoologists will continue to recognise them. Any problems that this may cause will simply have to be solved.

(In the longer term, a move to a more structured format is indeed desirable – in part, in order to facilitate automatic processing of nomenclatural acts and opinions. Such a change may be facilitated by providing PDFs alongside the structured form during the transition period. PubMed Central has gone some way towards making this possible by establishing a standard XML format which it recommends for depositions (the NLM Journal Publishing format, <http://dtd.nlm.nih.gov/publishing/>). Papers published in PLoS journals are available for download in this format as well as PDF.)

The current rules are too hard to get right

The respected online journal *Palaeontologia Electronica* (sponsored by the Palaeontological Society and the Society of Vertebrate Palaeontology among others) has published new names including the sauropod dinosaur *Karongasaurus* (Gomani, 2005). As described by the journal's nomenclature statement (Anonymous, 2007), ten copies of each issue are printed and deposited at ten archive libraries, which meets the requirements of the Code. However, this is done for the benefit of the ICBN, which does not recognise CD-ROM as a valid medium of publication under any circumstances, as the journal covers palaeobotany as well as palaeozoology. The nomenclature statement says that 'the CD-ROM issue of *Palaeontologia Electronica*, to be deposited at a minimum of five archive libraries, provides a permanent record that meets the requirements of the ICZN (Article 8.6) for valid and effective publication'. In fact, articles such as Gomani (2005) are not validly published, as the individual articles in *Palaeontologia Electronica* do not contain the necessary statement about copies being lodged in five named libraries. The statement continues, 'the ICZN recommends that formal nomenclatural citations should be made to the CD-ROM edition because of the inalterability of that medium', but in fact the CD-ROM edition is not published at all according to the requirements of Article 8.6.

This may seem a fine point, but it illustrates the larger issue that the current rules regarding electronic publication appear complex and arbitrary, and are difficult to get right even for journals that make the attempt. We have already seen how *PLoS ONE*, until recently, simply ignored the Code's provisions regarding electronic publications, and how the *Proceedings of the Royal Society B* continues to publish names online ahead of their subsequent valid publication in print. We now see that *Palaeontologia Electronica*, wishing to fulfil the requirements of the Code in good faith, nevertheless inadvertently recommends citation of a manifestation of its papers that are, according to the Code, not published. The upshot is that almost every citation of names published in *PLoS ONE*, *Proceedings B* and *Palaeontologia Electronica* is technically incorrect. This being so, we must ask ourselves: are all these journals really in error? Or is it the Code itself that is out of alignment with modern reality?

Conclusion

While we were looking the other way, the digital revolution has happened: everyone but the ICZN now accepts electronic publication. The Code is afforded legitimacy by workers and journals only because it serves them; if we allow it to become anachronistic then they will desert it – or, at best, pick and choose, following only those provisions of the Code that suit them. Facing this reality, the Code has no realistic option but to change – to recognise electronic publishing as valid.

I have no detailed recommendations to make regarding the recently proposed amendments to the Code (ICZN, 2008). Instead I ask only this simple question: will the Code step up to the plate and regulate electronic publications as well as printed publications? Because this is the only question that remains open. Simply rejecting electronic publication is no longer a valid option.

Let's not be overtaken by the rush of events. Eyes open, face into the wind. Let's go.

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Comment on the proposed Amendment of Articles of the *International Code of Zoological Nomenclature* to expand and refine methods of publication

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Giuseppe Manganelli, *Università di Siena, Italy* (Mollusca)

Sergio Martínez, *Universidad de la República, Montevideo, Uruguay* (Echinodermata)

Massimo Meregalli, *University of Turin, Italy* (Coleoptera, CURCULIONIDAE)

António Monteiro, *Lisboa, Portugal* (Gastropoda, CONIDAE)

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José Stuardo, *University of Concepción, Chile* (Mollusca and Crustacea)
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A list of 39 additional signatories can be found at http://www.iczn.org/Welter-Schultes_et_al_Additional_signatories

We have a critical view on the proposed Amendment of Articles of the *International Code of Zoological Nomenclature* to expand and refine methods of publication.

1. Electronic publications are not sustainable (technically yes, but not practically) and should not be accepted for nomenclatural acts, no matter which file format or version.

We have come to the conclusion that it is currently not possible to reliably maintain electronic data. Zoological nomenclature cannot work without the availability of printed original publications. From our experience we know that consulting 200–300 year old original literature is indispensable for taxonomic work. We cannot rely on secondary sources (see also 3). Some of us work almost exclusively with electronic files, but in most cases the file is a copy of a printed book.

AnimalBase is part of the consortium of the BHL-Europe project (Biodiversity Heritage Library for Europe), where IT technologists are currently trying to develop strategies for sustainable archival systems for electronic literature data, with the most modern methods and funded with millions of Euros by the European Union. Sustainable archiving consists of 3 components: ingest (= input), storage and digest (= consumption of data or output). Printed books are able to cover all three components. Since the mid-1450s this has accounted for successful long-term archiving and has minimised information loss. In digital environments these components must be strictly separated. If this is not understood, massive losses of information can result, as for example in the case of NASA's moon mission files, which are still present but that nobody can read any more, so that the information collected was almost entirely lost. The same applies to 20 % of the data saved in the Voyager spacecraft missions from the end-1970s. The problem is not necessarily of storage media, financial power, lack of IT specialists or early stage of information technology. Lack of an appropriate strategy, unawareness of the need to use a well selected storage format, unawareness of the need of metadata standards (the moon mission files were saved without metadata), and, very important in our case, sociopolitical issues, are the major threats to sustainable archiving.

In the proposed Amendment (Article 8.1.3.1) 'widely accessible electronic copies with fixed content and format' is mentioned as the only technical criterion to be

satisfied, and PDF is mentioned as an example. Nothing is mentioned about metadata requirements. Nothing is mentioned on strategies for storage format issues. PDF is an almost pure digest format that can currently be decoded because reader software is commercially available. All IT technologists working in the BHL-Europe project agree that PDF is not an appropriate format for storage. No one can guarantee us that in 20 years PDF will be the commonly used format, and that in 100 years a librarian will know at all what a PDF format was. In other words, relying on e-only publications in PDF format alone – without providing a strategy to solve the storage problem – bears the threat (or perspective) of irreversible and massive data losses in the future. LOCKSS and Portico, mentioned by the Commission as examples for permanent archives for electronic journals, have no strategies involving thoughts on sustainable storage formats and we do not consider them as promising approaches to solve the problem.

The storage component in an electronic archival environment is not trivial. The strategies currently developed in the BHL-Europe project involve building a gigantic computer terminal in England as a repository, in collaboration with leading IT companies such as Microsoft, IBM and others, and also the presence of administrative bodies encharged with replacing the storage formats in due time, without information loss, with new formats. We are currently in a situation in which the first steps for sustainable archiving are being developed. Long-term preservation of electronic information continuously requires high financial inputs, and it is still unclear who will pay the costs in the future. The problem is much less of a technical than of a sociopolitical nature. We are at the very beginning of a new age, and at a stage where we cannot predict that it will be possible to successfully preserve electronic biodiversity literature. The problem that, due to high costs, we will have only very few central repositories (only U.K. is funded, U.S.A. and China are planned) remains unsolved.

2. CD-ROMs and DVDs should not be regarded as published work, no matter when they were issued. This should also apply to CD-ROMs and DVDs issued after 1999 under Article 8.6 of the 4th edition of the Code. All authors who published on CD-ROMs and DVDs should be obliged to publish their nomenclatural acts on printed paper, and the publication dates would be that of the first publication on printed paper.

Firstly, CD-ROMs and DVDs cannot be read anymore after 100 years. There can be no doubt that the files will have become corrupted and there will be no machines to read them. Secondly, no name established since 1999 can have become so long-accepted as to provide a threat to the stability of nomenclature. And thirdly, authors who violated the Code's expressed Recommendation (8B) cannot expect that their actions will be protected for all eternity.

3. To be published, a work should obligatorily have been printed in a minimum of 100 paper copies. Publications issued after 1985 in less than 40 copies should not be regarded as published work. Works of which no original is available anymore should not be recognised as published work.

This would reflect current practice in zoology, where doctoral and diploma theses issued in a few offprints are usually not accepted for nomenclature. Doctoral theses in countries where 50 paper issues were required to be given to a public library are probably recognised by most taxonomists as published work. The Code does not currently reflect taxonomists' behaviour in this regard and there is an urgent need to

update the Code to comply with recent technological developments. Since home printers became available it has become technically possible to print out 2 sheets and declare this as Code-compliant published work. A minimum number of required copies is urgently needed in response to technical progress. In AnimalBase we are currently not aware of a single case where it is certain that a printed work is not available anymore. We know some cases cited in secondary sources where some taxonomists suspect that the originals were manuscripts, and others argue that these had been publications.

4. Journals publishing primarily electronically should contain a statement printed at least once in each issue that printed copies on durable paper of every issue are deposited in a minimum of 30 major libraries which are identified by name in the volume itself. The issues should also actually be deposited in these libraries.

The need to meet this obligatory requirement would be helpful for taxonomists to select journals for submitting papers with nomenclatural acts. Zoologists working in certain fields can subdivide their publications, to publish bioscientific contents in e-only journals and nomenclatural acts in paper-based journals. In botany this is the same situation. We see advantages in a slightly reduced number of journals in which nomenclatural acts can be published. Nomenclatural acts should preferably not be published in non-taxonomic journals, the reviewers of which are not necessarily skilled to evaluate correctness in nomenclatural matters.

5. The Commission should not have the power to declare unpublished work (including electronic publications) as published work; neither should it have the right to issue Declarations on the matter of what constitutes published work.

6. The Official Register should not be mentioned in the Code.

We personally appreciate very much the efforts to establish ZooBank, but it should only be officially included in the Code after one or several decades of positive experience, showing how such a system would be maintained running effectively without any financial background provided by the zoological community. We need to see its powers, its limitations and its acceptance by the whole zoological community, including those who do not speak English and who do not participate in international discussions.

7. In particular, we recommend consideration of the following suggestions regarding the proposed modifications of the Code:

The proposed new Articles 8.1.3.1 and 8.1.3.2 should not be included. Article 8.1.3. should be modified: *'8.1.3. it must have been produced in an edition containing simultaneously obtainable copies by a method that assures numerous identical and durable copies, and it must be extant in at least one surviving original copy.'*

Article 8.5 as proposed should not be included. Article 8.5 should be modified:

'Article 8.5. Works issued after 1985. To be published, a work must have been printed on durable paper (by either letterpress or offset printing, newspaper and similar paper quality is excluded) in a minimum of 40 copies. Works issued after 2010 must have been printed on durable paper in a minimum of 100 copies.'

Article 8.6 should be modified:

'Article 8.6. Works issued and distributed electronically. All works produced by a method other than printing on paper, including works issued and distributed electronically in digital formats, are not regarded as published work. This applies expressly also to CD-ROMs and DVDs issued after 1999 under Article 8.6 of the 4th edition of

the Code. To be recognised as published work, journals publishing primarily electronically (including open access journals) must contain a statement printed at least once in each issue that printed copies on durable paper of every issue are deposited in a minimum of 30 major libraries which are identified by name in the volume itself. The issues must be deposited and obtainable in these libraries.'

The proposed new Article 8.6 should not be included.

Article 9 should not be modified as proposed, except 9.9 which could be modified as proposed under Article 9.10.

Article 9.8 should be modified:

'9.8. information issued and distributed by means of electronic signals'

The proposed new Article 10.8 should not be included.

To promote stability of a widely used and well-known name established on CD-ROM or DVD after 1999, the Commission has the right to make available such a name from the next occasion when the name was published on paper after the CD-ROM was issued and where the explicit statement that this was a new species (violation of Article 16.1) is lacking.

The proposed new Article 10.9 should not be included.

The proposed new Article 21.8.3 should not be included.

The proposed new Article 21.9 should not be included.

The proposed new Article 78.2.4 should not be included.

Lots of Copies still need the focal function of libraries

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'Lots of Copies keeps stuff safe' has been the argument in principle and suggested specific mechanism (www.LOCKSS.org) for archiving electronic-only publications.

Libraries have served the archival role and journals the multiple identical copies so well for so long that they have been taken for granted. Electronic publications are clearly elbowing out print journals, but their longevity and unchangeability are not so certain (notwithstanding the opinions of some active contributors to the ICZN online discussions; despite requests, the archiving industry has yet to comment on this lively discussion). The framers of the 4th edition of the Code, seeing change on the horizon, allowed for publication of nomenclatural acts on CDs, while prohibiting them online. This was probably seen as a sort of link with print and likely permanency in that there was a physical object. We now know that CDs are less permanent than had been thought, and so part of the amendment currently being considered is to prohibit them henceforth as a medium for nomenclatural acts. Of at least as great concern is archiving. This is also relevant to the use of CDs because many libraries have no provision for archiving one-off CDs in the way ICZN4 stipulates. As for electronic products in general, it may be true, as many contributors to the ICZN online discussions have argued, that with so many copies out there, continued existence of electronic publications is not at issue, and so libraries need no

longer be a consideration. But one of my concerns is how a scientist new to a field can find the publications.

Presumably that person has to know who holds the most complete library of electronic copies (disregarding whether they are PDFs, Word documents, or in some other format). There is a precise analogy in the print world – many of us have extensive personal reprint collections, which have never been proposed as replacements for libraries. Libraries are gathering places, central points for finding information. The electronic world is dispersed, and we have all heard stories about what large proportion of relevant information search engines miss, so the focal function of libraries is another piece of the nomenclatural puzzle (and a reason a central register for names is being considered). We are in early days, and there is as yet no obvious equivalent for many of the multiple roles libraries have performed that were so much taken for granted they were not even mentioned in the Code! Now we have to recognise explicitly what we need and how to go about organising our science and information in this new world.

Open access publications and archiving

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In the area in which I work (biology of salticid spiders), the major taxonomic references are already online (Platnick AMNH, Prószyński, see <http://www.peckhamia.com> for links).

Increasingly, documentation by photographs rather than only preserved specimens is becoming important, and we have open questions about this. Jerzy Prószyński (<http://www.gsd-salt.miiz.waw.pl/salticidae.php>) has taken the lead in this area, and he and I have been trying to resolve related questions. What we really want to be able to do is serialise (collection; serial number) these photographs for standard reference. We have issues to work out. I think we need a standard repository for serialised photographs, and I think that all accepted photographs need to be placed in the public domain at the time that they are posted, so that they can be freely referenced and incorporated into future work. It's that simple.

If you review some of the earlier taxonomic work (as I have recently, in considerable detail), you find that all descriptions and drawings carried a significant measure of ambiguity (e.g. what is really meant by 'testaceous' or 'rufous'?) that is resolved in photographs. Photography is now generally available and highly affordable (major change). Preserved specimens also lose many details (posture, coloration) as they are 'rubbed,' and are more difficult to link to field work than are photographs of living specimens. We also have the ability, with photography and computers, to produce almost unlimited depth of field in photographs of specimens. Peer review does not have to take place before posting or 'publication'. It is an ongoing process in science, as more information becomes available. We have no problem with the dynamic process here. Even the taxonomic work of 'highly authoritative experts' gets reversed as more knowledge becomes available. That is fine. In the trade-off, we want more interest and participation in field zoology, not

less. Right now, instead of a handful (about 10) people with an interest in the identification of jumping spiders (SALTICIDAE), we are getting the interest and participation of hundreds of people worldwide, many close to remote localities and equipped with very sophisticated digital cameras. The 'old systematics' never had these tools or this broad participation.

'Private' publications (not owned by journals or publishers) have always been a reality. Some of the best and most motivated work can be found here. The problem is not private publication itself but commercialisation and control, even by 'non-profit' societies. All new species descriptions, naming, and opinion should be posted or published under 'no copyright, open access' terms, in free repositories. The few places where I have had an issue with private publication are where the publishers wanted to charge for access to their work (in some cases it is even hard to know that this work exists). I don't think that names published under these conditions should be acceptable. The ICZN should open a limited, five-year window for all *older* scientific names to be re-filed (with reference to older publications) in an open-access repository, but at the same time require all new scientific names and related publications to be posted in a new open-access archive. I am confident that the international community that studies jumping spiders would enthusiastically support this window.

Naming is a very large, multi-year project. This should be viewed as a *collaborative* project in the future, not as a source of private ownership of related works. Naming exists so that we can all communicate with each other.

On the proposed amendment of the Code

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I strongly agree with this amendment. As pointed out in the proposal, archiving technology is now in place to ensure the survival of electronic data, even if no paper copies are available. I do not agree with Welter-Schultes et al. that electronic data are so ephemeral that their survival is much more dubious than paper copies.

Comment on the Commission's proposal to amend the Code to expand and refine methods of publication

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As for the Commission's Proposal to amend the Code concerning allowed publication methods (BZN 65: 265–277), I basically share the objections lodged by Welter-Schultes et al. (this issue): the currently available electronic archiving solutions are still in their initial technical stages and thus neither the physical

permanency of the storage media nor the long-term readability of the archived files by future software and hardware is ensured.

Almost every present-day zoologist will recall 'floppy disks' and punch cards. Has anybody tried to find technical devices to read them recently? Even larger libraries or archives will fail to provide the necessary technology. Today, diskettes and even CDs or DVDs are following these forerunners on their way to oblivion, and there is little reason to believe this technical development has stopped. As for the number of copies available, it is without doubt substantially bigger in the case of popular music tracks than in, say, the proceedings of a zoological conference of the early 1990s. But even if we found the file and the machinery to read it, we would need the right software to open it. PDF/A is the first software designed to be 'self-contained' (not needing additional software) and may possibly become a standard for the future. However, at present we see that none of the common online journals makes any consistent use of PDF/A. They may indeed have good reasons not to do so, e.g. the large size of each data file, however, for the challenging purposes of taxonomy their approach must be deemed insufficient. Even if almost all of the taxonomical community agreed with this viewpoint it wouldn't stop publishers of online journals feeling that their publications do already fulfil the requirements of the suggested amendment to the Code, for their contents would exhibit 'fixed content and format' (proposed Article 8.1.3.2). The Commission named PDF/A only as an example for good reasons, since other archival formats are likely to be developed—however, this necessary wording obviously opens the door for arbitrary interpretations.

Even if we could solve most of the storage, transformation and reading problems technically, these solutions would always depend on human beings willing and able to finance and execute such steps. Printed works have been available and readable for centuries, and present-day prints can be expected to be likewise for at least as long. The work of archiving printed books or journals is certainly a much less demanding task than preserving data files and keeping them accessible. However, nobody would doubt the advantages that modern information technology provides to science, and we biosystematists would not like to do without it. The question therefore is not whether one should print works 'traditionally' or instead distribute them electronically. Rather one has to find the best way of combining the advantages of both kinds of technology. For the time being I would strongly suggest that printed publications should form the main reference for zoological systematics, given their huge advantage of storing data as published more or less permanently and also of providing easy access. Currently it would be premature to include any kind of digital publication as a taxonomic reference in the Code.

Electronic publications are encouraged for the benefit of science, however, in order to address taxonomic matters, any such work must be available in print, with only the printed version providing the taxonomic reference. With technologies such as print-on-demand, which could be used to comply with the Code, the cost of producing a smaller print-run based on an existing electronic dataset is very low, so this would not provide any major impediment for the organisations publishing online journals. Each electronic version should contain detailed information on its identity and, if the printed edition of the paper is already available, a concise reference to this fact should also be given. It would be desirable, however, and not difficult to ensure that the print and electronic versions of the paper were published simultaneously so

that each of them could include a statement like: 'This paper has been published (in the journal *XYZ*) on 1 April 2010 in accordance with the requirements of the International Code of Zoological Nomenclature.' Such information could be checked sporadically perhaps by ZooBank or another suitable body within a short period of time, e.g. one year, and where publishers could not then verify their statement, the journal or book would be considered unavailable.

I would not follow the suggestion of the Welter-Schultes et al. to have special conditions for the print edition of electronic journals which, as in their suggested revision of Article 8.6, would make both the practical procedure and the Code unnecessarily complicated. I would also modify their suggested Article 8.5 on another fundamental point: whereas there are no technical specifications for publications issued after 2009 other than the use of durable paper, for earlier publications all printing technologies apart from 'letterpress or offset printing' are excluded. This *inter alia* applies to print-on-demand technologies, which I referred to above quite positively. Print-on-demand publications can be produced with an initial print run large enough to fulfil the requirements of the Code, and a statement that they are in accordance with the Code can easily be included in the publication. In its original Proposal the Commission rightly noticed the possibility of silent changes, however, if the editors / publishers are recommended by the Code not to allow these and to name modified versions appropriately e.g. as secondary editions of the respective work, I don't see any particular reason for concern about the issue. In general I approve of the Commission's proposal to accept any printing 'using ink or toner' (Article 8.4.1), although I would add the word 'durable' before 'paper' and again before 'ink or toner'.

Although CD and DVD publications were allowed by the recent edition of the Code it is now understood that this was a mistake. Given the deterioration rate of these media, some of the CDs and DVDs of that period are by now damaged and unreadable, whilst others are not expected to survive for more than a century. Thus it is not in the interest of stability of zoological nomenclature that these media should be regarded as available publications any longer. However, since some such publications do exist, we can not merely regard them as unavailable, as Welter-Schultes et al. propose. I would suggest that the Commission invites all authors so affected to redescribe the taxa they originally published electronically within a limited period of, say, two years. This should be done with a publisher's statement that this new description is deemed to replace an older one made public electronically only, and that s/he has personally seen this original publication. The Commission should rule that such replacement descriptions are considered available with the date of the print publication and should be accepted as valid names even if they are pre-dated by descriptions of the same taxon published between the dates of the original and replacement publications. However, in the event of the stated date for the electronic publication being shown to be wrong in that it actually post-dated the additional description, this declaration would not come into force and the earlier printed publication would become valid. In such cases a separate ruling of the Commission would be necessary given a time limit of, say, five years to apply for such a ruling. In order not to overstress the Code I suggest that the Commission deals with the issue in a separate declaration and not in any Article of the Code. Therefore I would prefer it if the present Article 8.6 were omitted in its entirety.

Generally, however, I consider it problematic for the Code to be changed a posteriori, as Welter-Schultes et al. propose (cf. the obligatory numbers of prints mentioned in their Point 3). One may doubt whether the suggested number of 100 copies is ideal for future publications. While this would better ensure preservation of copies, many under-financed scientific organisations publishing their own small journals might prefer a smaller number such as the Commission's suggestion of 25 copies.

For the time being, I would suggest printed publications to remain the only acceptable reference for zoological systematics. Currently it would be premature to include any kind of digital publication as taxonomic reference in the Code.

Nomenclatural consequences resulting from the rediscovery of *Les figures des plantes et animaux d'usage en médecine*, a rare work published by Garsault in 1764, in the zoological literature

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Abstract. The zoological part of the rare medical work *Les figures des plantes et animaux d'usage en médecine* published by F.A.P. de Garsault in 1764 should be considered as binominal and contains dozens of generic and specific names that were new at the time. Several generic names entered zootaxonomy in the early period (the fish names *Merlangius*, *Trutta* and *Ichthyocolla* are attributed to this work and still used), but apparently this rare work later got lost in the zootaxonomical record. Since the 1830s Garsault's work and names in zoology were attributed to the physician E.F. Geoffroy, who was mentioned on the title page but clearly not as the author of the work, and later to the zoological author E.L. Geoffroy. With this incorrect authorship zoologists could no longer find Garsault's work in library catalogues or botanical bibliographies. The rediscovery of this work has a significant impact on some thirty well-known animal names, mainly vertebrates. Nine widely used names should be conserved under the plenary power, and 16 others should take Garsault's (1764) authorship (*Alosa*, *Anguilla*, *Asinus*, *Bufo*, *Coturnix*, *Cygnus*, *Martes*, *Merlangius*, *Rupicapra*, *Salamandra*, *Scincus*, *Tinca*, *Trutta*, *Vipera*, *Vulpes*, *Castor canadensis*). In an annotated list of names we propose how to proceed with the problematic names established by Garsault (1764). This case demonstrates that the author's name should always be cited as in the original work, that authorships for names of taxa should not carry initials of first names, and that any deviation from the original name may lead to misunderstandings and in the worst case to a complete disconnection with the original source of information.

Keywords. Taxonomy; nomenclature; Vertebrata; Geoffroy; Garsault; authorship; subsequent absolute tautonymy.

Introduction

In the course of compiling literature lists for a project of digitising early zoological literature (AnimalBase), an almost unknown early work published by F.A.P. de Garsault in 1764 was discovered (Welter-Schultes et al., 2008). Only 50 zoological publications in which new binominal animal names were established are known between 1757 and 1765.

Garsault's work *Les figures des plantes et animaux* consisted of one text volume and five plate volumes (four on plants, one on animals). It is rare in public libraries, but was listed in the renowned botanical catalogues of Nissen (1966, p. 65; 1969, p. 155) and Stafleu & Cowan (1979, p. 919) under Garsault's authorship. In zoological sources (Sherborn, 1902; Neave, 1939–1940), cryptic abbreviations of the title and some names for taxa described therein were listed under the incorrect authorship

of Geoffroy. After 1837 Geoffroy was used as a standard zoological author's name for Garsault's work and its corresponding taxa. This and the confusion between various authors referred to as 'Geoffroy' provided a serious obstacle for zoologists to find Garsault's work again. We discovered it in a library catalogue by screening electronic title files with Sherborn's (1902) cryptic data ('Descr. 719 Plantes, & c.'), neglecting the author's putative name E.L. Geoffroy. Zoologists must once have known Garsault's work, because several generic names with E.L. Geoffroy's erroneous authorship combined with the date 1767 are known, at least one (the fish genus *Merlangius*) being currently used at the generic level. The generic fish names *Ichthyocolla*, *Lucius* and *Trutta* have also been recently used, also partly attributed to E. Geoffroy Saint-Hilaire.

Historical context and issues of Garsault's work

Here we give a short summary of the history of Garsault's work. For more detailed information on the historical and bibliographical background see Welter-Schultes et al. (2008). François Alexandre Pierre de Garsault (1691–1778) was a botanical artist and naturalist who worked in Paris where he published several works on horses, economy, trade and commerce (Garsault, 1741, 1746, 1756). In 1764 he published *Les figures des plantes et animaux d'usage en médecine*. This contained 729 copperplate engravings, made from his own drawings. Garsault's work formed part of *Matière médicale*, a multi-volume work issued in several parts from 1741 onwards and attributed to the physician Étienne François Geoffroy (1672–1731). *Matière médicale* contained zoological parts compiled by Arnault de Nobleville and Salerne (1756–1757). Many animal species were represented there with long descriptions of medicinal receipts made from products derived from these animals. Linnean zoologists never referred to these volumes of *Matière médicale*.

Garsault compiled five volumes, four on plants (plates 1–643) and one on animals (plates 644–729). Each organism was given a Latin scientific and a French vernacular name. The animals were arranged systematically, beginning with molluscs and arthropods, followed by fishes and whales, amphibians, birds and mammals. Garsault added a 20-page index for the whole work in which the Latin and French names were repeated and arranged in order of appearance in the plate volumes. The combined work was published in June 1764. On the title page Garsault was clearly given as the author, and E.F. Geoffroy's name 'Mr. Geoffroy médecin' was mentioned as the original editor of *Matière médicale*.

After Garsault had issued his plate volumes in June 1764, readers demanded accurate descriptions of the plants and animals to enable them to understand the illustrations (Garsault, 1767), so he decided to compose a text volume containing descriptions and basic medical information for each organism. This volume, *Explication abrégée*, appeared in 1765. Garsault followed exactly the arrangement of the plate volumes, repeated exactly (with few exceptions) the Latin and French names for each organism, noted which parts were used for medicinal purposes, added brief descriptions, explained where the organisms were found, and finally listed which pharmacological products were made from them. The content was only partly derived from Arnault de Nobleville and Salerne (1756–1757), so Garsault must have had access to other sources of zoological information.

Two years later, in 1767, the two parts (text and plates) were re-issued, receiving a new title, *Description, vertus et usages*. Here also Garsault was given as the author and 'M. Geoffroy' was mentioned as the editor of *Matière médicale*.

Binominal zoological work

Garsault used polynominal names in the botanical part, and today the specific names published in Garsault's works are suppressed (Stafleu & Cowan, 1976, p. 919; McNeill, 2006, p. 482). Surprisingly, all zoological names were consistently binominal. Garsault's Latin names for the animals were either uninominal (genus only) or binominal (genus and species). Those which were polynominal in Arnault de Nobleville & Salerne (1756–1757) were converted to binominals. Classifying the zoological part as non-binominal would not be justified because no polynominal names appear in volume 5 or in the index.

There is no direct evidence that Garsault had consulted Linnean zoological literature, but he must have had access to sources and ideas of the Linnean system. Welter-Schultes et al. (2008) suggested that Garsault obtained some basic Linnean zoological knowledge from Louis Jean Marie Daubenton (1716–1769), who had serious disputes with Buffon in the 1760s (Buffon strictly rejected the Linnean system and did not allow his co-workers to use Linnean names) and who was among the first French zoologists publishing binominal names in the 1780s.

The structure of Garsault's binominal names

Doubtless, although he did not use polynominal names, Garsault (1764) did not understand the Linnean system properly. He considered specific epithets necessary only in case of doubt, otherwise he used the generic name alone. Frequently he simply changed the rank from specific to generic level, probably without even understanding the consequences of this procedure. His marginal understanding of scientific nomenclature, also demonstrated in the botanical part with its many polynominals, resulted in the establishment of numerous new generic names. Garsault's method however was not uncommon at the time and was also used by Linnean zoologists. Later authors coincidentally used the same generic names as Garsault (1764), applying the same method when borrowing Linnean specific names for the generic level.

Some Latin names were questionable. On planche 707 showing a stag and a hind, the names used were *Cervus* for the male but *Cerva* for the female. Likewise, *Bos* was given for a bull but *Vacca* for a cow (pl. 699), and, for humans, *Homo* (male) and *Mulier* (female) (pl. 729). All of these are the appropriate Latin vernacular names, yet *Cervus*, *Bos* and *Homo* are also the correct Linnean generic names. The Latin feminines were probably engraved on the plates by engravers on their own initiatives and not meant as scientific names (Welter-Schultes et al., 2008). When compiling the text volume, Garsault seemed to have realised that these pairs of names did not meet scientific standards and he did not repeat the feminine names. Instead, Garsault (1765) listed '*Bos*, Taureau, Vache, Veau, Bœuf' (p. 442), '*Cervus*, Cerf' (p. 451), '*Homo*, Homme, Femme' (p. 471), underlining that these were the Latin terms that were meant as scientific and not as vernacular names.

The marine snail known as *Turbo rugosus* Linnaeus, 1767 (spelled as Linné) was presented in three different orthographic versions: '*Cochlea cœlata*' on the engraving (Garsault, 1764, pl. 644), '*Cochlea celata*' in the index (p. 17) and '*Limax cochlea*

cæolata' in the text volume (Garsault, 1765, p. 376). The last name is not a polynominal name, the term *cochlea* has to be interpreted as a subgenus. A polynominal name consisted of one genus-like name combined with several adjectives, never of two genus-like names and one adjective. It is remarkable that Garsault (1764) created such a combination, which supports the assumption that he had access to a source of information about the basic principles of binominal nomenclature.

Zoological perception of Garsault's work

While botanists and general bibliographers have always attributed the work to Garsault, zoologists did this only initially (Walbaum, 1788, p. 114; Engelmann, 1846, p. 168). In zoological sources the work was later associated with E.F. Geoffroy (Percheron, 1837, p. 131; Hagen, 1862, p. 270), and with E.L. Geoffroy (Engelmann, 1846, p. 168; Sherborn, 1902, p. xxv). As already noted, the incorrect authorship made it impossible for zoologists to find the work in library catalogues and botanical bibliographies. In this way the information contained in Garsault (1764) got lost and only some generic names survived. Garsault never published anything else that entered zoological nomenclature, so his name has remained completely unrecorded in the zootaxonomic field.

The fact that the physician and invertebrate zoologist Étienne Louis Geoffroy, who began publishing in 1762 and is sometimes referred to as 'the younger Geoffroy' (Martini, 1767), had the same first and last names as Étienne François Geoffroy, to which *Matière médicale* was attributed, contributed to misunderstandings among zoologists. Sherborn (1902) listed the following names and attributed them to E.L. Geoffroy, 1767: *Asellus* ('Crustacea'), *Capricerva*, *Cetus*, *Cynos*, *Harengus*, *Ichthyocolla*, *Lucius*, *Merlangius*, *Mulus*, *Ranetta*, *Tinca*, *Trutta*. Although there is a copy in the British Library, Sherborn probably never saw Garsault's work. Had he consulted the original work he might have perceived that *Asellus* did not denote a crustacean, but a fish.

Neave (1939–1940) largely copied Sherborn's data, omitted *Asellus* and *Cetus*, but added *Martes* to the list of names attributed to E.L. Geoffroy, 1767. Since *Martes* was added, there must be other sources in zoological literature where Garsault's names survived, next to those that Sherborn (1902) consulted. Zoologists have never considered Townsend's (1956) short note that not E.L. Geoffroy but E.F. Geoffroy had been meant.

The confusion about Geoffroy's name is completed by the existence of a third zoologist Étienne Geoffroy Saint-Hilaire (1772–1844), a professor of zoology and anatomy at the Muséum d'Histoire Naturelle in Paris in the mid-1790s, who published many important zoological works including descriptions of many new mammalian taxa from 1795 onwards into the early 1800s. In his first articles in the *Magasin Encyclopédique*, published in co-authorship with Georges Cuvier, his name was given as 'citoyen Geoffroy', later as 'Ét. Geoffroy'. Sherborn (1902) referred to this author as 'E. Geoffroy'. During the process of an intended standardisation of names of authors the fish *Ichthyocolla* was combined with Geoffroy Saint-Hilaire, 1767 (Catalog of Fishes, online version 19 September 2008, entry *Huso*). Finally, his son Isidore Geoffroy Saint-Hilaire (1805–1861) was also a zoologist.

Possibly the first zoologists to verify Garsault's original work after Hagen (1862) were Bogutskaya & Naseka (2004, p. 156) when attempting to verify the authorship

of *Trutta* 'Geoffroy, 1767'. Consulting Hagen (1862), Bogutskaya & Naseka (2004) realised that not E.L. Geoffroy but E.F. Geoffroy was meant and that the work, contrary to Sherborn's (1902) entry '1767', dated from June 1764. It was the first time that a genus described in Garsault's work (*Trutta*) had been combined with the 1764 date, albeit again not with Garsault's authorship.

Annotated alphabetic list of Garsault's most problematic generic and specific names

The names established by Garsault (1764) referred to well-known animals of medical importance. Most animals had already been named by Linnaeus (1758).

According to Article 12.2.7 of the Code the proposal of a new generic name in combination with an illustration of the taxon satisfies the criterion of an indication, so Garsault's (1764) names were correctly established. It is implicitly (but not explicitly) ruled in the Code that only those specific taxa which do not contradict the figure are potentially eligible as type species for a genus established without included species. In the case of Garsault's names, we are usually dealing with one single candidate species for which the generic name was established.

Garsault (1764) listed 37 new generic and 30 new specific names. Most specific names represented junior synonyms of Linnean species. In some cases Garsault's (1764) names were senior homonyms of younger names that were later established by other authors. Where appropriate, we provide a guide for the measures to be taken in order to protect the currently used names for the figured animals. It was not possible to research the methods of fixation of all type species involved, this should eventually be done by specialists of the corresponding animal groups, who might also find more junior homonyms. We did not find any instances of usage for the names *Anas sylvestris*, *Anser domesticus*, *Aquila regalis*, *Cervus rangifer*, *Lacerta terrestris*, *Mus major*, *Mus minor*, *Rana viridis*, *Turdus minor* and others, all described by Garsault (1764) but possibly never again mentioned, or at least not in use today.

We understand the term 'stability of names' used in the Code to mean the stable use of generic or specific names themselves, excluding authorship or date. In some cases years and authors for names on the Official List must be corrected (*Anguilla*, *Antalis*, *Coturnix*, *Rupicapra*, *Vulpes*). We believe that an earlier date of publication for a name contributes to stability.

Alce Garsault, 1764, pl. 709 (Mammalia)

Appeared without species. Text volume: Garsault, 1765, p. 452. Garsault's figure represents the elk or moose, *Cervus alces* Linnaeus, 1758, currently classified in the genus *Alces* Gray, 1821 (Opinion 91 (1926), type species *Cervus alces* Linnaeus, 1758 by absolute tautonymy). *Alce* Blumenbach, 1799 was established for the giant deer (type species *Alce gigantea* Blumenbach, 1799 by monotypy) so it is a different genus and cannot be regarded as a subsequent use of *Alce* Garsault, 1764. *Alce gigantea* Blumenbach, 1799 is currently classified either in the genus *Cervus* Linnaeus, 1758 (type species *Cervus elaphus* Linnaeus, 1758 by Linnean tautonymy), or in *Megaloceros* Brookes, 1828 (type species *Megaloceros antiquorum* Brookes, 1828 by monotypy, regarded as a junior subjective synonym of *Alce gigantea* Blumenbach, 1799, Opinion 1566 (1989)). *Alce* Blumenbach, 1799 is currently not used (Nowak, 1999, p. 1093). *Alce* has never been used in Garsault's (1764) sense, and specific names have probably never been included in *Alce* Garsault, 1764.

If *Cervus alces* Linnaeus, 1758 or one of its synonyms were to be designated as type species of *Alce* Garsault, 1764, which would be the only species matching this genus, then *Alce* Garsault, 1764 would become a senior synonym of *Alces* Gray, 1821. This problem could be solved under Article 23.9.2. But it is also justified to consider *Alces* as an incorrect subsequent spelling for *Alce* Garsault, 1764, because the same animal was meant.

Alce Blumenbach, 1799 has not been used for the giant deer, although it clearly pre-dates *Megaloceros* Brookes, 1828. Since *Alce* was established for the elk by Garsault in 1764, *Alce* Blumenbach, 1799 is not available, being a junior homonym of *Alce* Garsault, 1764.

If *Alces alces* (Linnaeus, 1758) remains in stable use for the elk, it should be protected by suppressing *Alce* Garsault, 1764 for the purposes of the Principle of Priority, but not for those of the Principle of Homonymy. Suppressing *Alce* Garsault, 1764 for the purposes of the Principle of Homonymy would also protect the name *Megaloceros* Brookes, 1828 for the giant deer.

***Anguilla* Garsault, 1764, pl. 661 (Actinopterygii)**

Appeared without species. Text volume: Garsault, 1765, p. 400. Garsault's figure represents the eel, *Muraena anguilla* Linnaeus, 1758, currently classified in the genus *Anguilla* Schrank, 1798 (type species *Muraena anguilla* Linnaeus, 1758 by monotypy, confirmed in Opinion 1672 (1992)). Garsault (1764) used the name *Anguilla* in the same sense as Schrank (1798).

The name *Anguilla* must be shifted to Garsault's (1764) authorship, type species *Muraena anguilla* Linnaeus, 1758 by subsequent monotypy by Schrank (1798). *Anguilla* as used by Schrank (1798, pp. 304, 307) can be considered as a subsequent use of *Anguilla* Garsault, 1764. An appropriate correction should be made to the Official List of Generic Names in Zoology, where *Anguilla* is attributed to Schrank (1798).

***Antalium* Garsault, 1764, pl. 646 (Scaphopoda)**

Appeared without species. Text volume: Garsault, 1765, p. 378. Garsault's figure represents *Dentalium elephantinum* Linnaeus, 1758, type species of the genus *Dentalium* Linnaeus, 1758, by subsequent designation by Montfort, 1810, p. 23 (not *Antalium* Guettard, 1770 (Polychaeta), a name that is probably not used). Sherborn (1922, p. 360) listed *Antalium* Herrmannsen, 1846 (Scaphopoda). Herrmannsen (1846, p. 63) listed *Antalium* 'Auctt.' without further reference or description as a synonym of *Antalis* Herrmannsen, 1846. *Antalium* Herrmannsen, 1846 has to be considered as a nomen nudum, and the name is currently not used.

In Opinion 361 (1955) *Antalis* Herrmannsen, 1846 was regarded as a nomen nudum, and *Antalis* Adams & Adams, 1854 as available (type species *Dentalium entalis* Linnaeus, 1758 by subsequent designation by Pilsbry & Sharp, 1897, p. 37). This was not tenable: *Antalis* Herrmannsen, 1846 had a short description and several literature indications (among others Bonanno, 1684, p. 91), and thus was made available under Article 12.1 of the Code. *Antalis* Herrmannsen, 1846 was established without included species. We think that the type species *D. entalis* matches Herrmannsen's (1846) very vaguely defined concept of the genus, and that the name *Antalis* must be shifted without further consequences to Herrmannsen, 1846.

Antalis is currently used by some authors as a genus or subgenus (Steiner & Kabat, 2004, p. 712), for a different group of species from the one that contains *D. elephantinum*. *Antalium* must be shifted to Garsault's (1764) authorship and *Antalium* as listed by Herrmannsen (1846, p. 63) must be considered as a subsequent use of *Antalium* Garsault, 1764. In the current classification *Antalis* and *Antalium* are not synonyms.

Any interpretation of Garsault's (1764) name to represent something other than a group of species containing *D. elephantinum* would be in contrast to Garsault's (1764) use of the name. If *D. elephantinum* Linnaeus, 1758 or one of its synonyms were to be designated as the type species for *Antalium* Garsault, 1764, then *Antalium* Garsault, 1764 would become a junior synonym of *Dentalium* Linnaeus, 1758.

***Cancer fluviatilis* Garsault, 1764, pl. 654 (Crustacea, Decapoda)**

Text volume: Garsault, 1765, p. 387. Garsault's figure represents the crayfish, *Cancer astacus* Linnaeus, 1758, currently classified in the genus *Astacus* Fabricius, 1775, type species *Cancer astacus* Linnaeus, 1758 fixed under the plenary power (Direction 12 (1955), Opinion 104 (1928)). *Cancer fluviatilis* Garsault, 1764 is a junior synonym of *Cancer astacus* Linnaeus, 1758. *Cancer fluviatilis* Herbst, 1785 is a well-known freshwater crab currently classified as *Potamon fluviatilis* (Herbst, 1785), and a junior homonym of *Cancer fluviatilis* Garsault, 1764.

If *Cancer fluviatilis* Herbst, 1785 should remain in stable use for the freshwater crab, *Cancer fluviatilis* Garsault, 1764 needs to be suppressed for the purposes of the Principle of Homonymy.

***Caprea* Garsault, 1764, pl. 704 (Mammalia, BOVIDAE)**

Probably not a misspelling for *Capra* Linnaeus, 1758, but more likely an independent name (the name *Capra* was used for the goat on pl. 703). Text volume: Garsault, 1765, p. 488. Type species *Caprea moschi* Garsault, 1764 by monotypy, which is a junior synonym of *Capra gazella* Linnaeus, 1758, currently classified in the genus *Gazella* Blainville, 1816 (ruled by the Commission under the plenary power to be available, Direction 23 (1955), type species *Capra dorcas* Linnaeus, 1758 fixed under the plenary power, as designated by Ogilby 1837, p. 137). At generic and subgeneric level according to the current classification (Nowak, 1999, p. 1199), *Caprea* Garsault, 1764 represents a senior subjective synonym of *Gazella* Blainville, 1816, and a senior homonym of *Caprea* Ogilby, 1837 (Mammalia, CERVIDAE). *Caprea* Ogilby, 1837 is a junior synonym of *Capreolus* Gray, 1821 and currently not used (Nowak, 1999). We cannot exclude that *Caprea* has been used as a name somewhere since 1899, so protecting *Gazella* under Article 23.9 could be ineffective.

If *Gazella* Blainville, 1816 should remain in stable use for the gazelles, *Caprea* Garsault, 1764 needs to be suppressed for the purposes of the Principle of Priority, but not for those of the Principle of Homonymy.

***Capricerva orientalis* Garsault, 1764, pl. 705 (Mammalia)**

Text volume: Garsault, 1765, p. 448. Type locality Iran, 'dans les montagnes escarpées de la Perse'. Garsault's figure represents the wild goat, *Capra aegagrus* Erxleben, 1777 (original and current combination). *Capricerva orientalis* Garsault, 1764 is a

senior synonym of *Capra aegagrus* Erxleben, 1777. This means that *Capra orientalis* (Garsault, 1764) is a possible candidate name that could be used for the wild goat.

If *Capra aegagrus* Erxleben, 1777 should remain in stable use for the wild goat, it should be protected under Article 23.9 against *Capricerva orientalis* Garsault, 1764. We assume that a name *orientalis* has not been used for these animals.

***Cochlea* Garsault, 1764, pl. 644 (Gastropoda)**

Text volume: Garsault, 1765, p. 376. Generic name listed by Sherborn (1902, p. 228), but (correctly) attributed to E.L. Geoffroy (1767, p. 12). Geoffroy (1767) was rejected (Opinion 362 (1955)) so the name is not available. Garsault established the generic name most probably independently from E.L. Geoffroy. The only specific name directly and unambiguously included in *Cochlea* Garsault, 1764 was *Cochlea coelata* Garsault, 1764, which therefore, according to Article 67.2.5, has to be considered as its type species by monotypy. *Limax terrestris* Garsault, 1764 (a junior synonym of *Helix pomatia* Linnaeus, 1758) was presented as '*Cochlea* seu *Limax terrestris*'; this was the second use of this name, but not unambiguous as required by the Code. *Cochlea coelata* Garsault, 1764 is a senior synonym of *Turbo rugosus* Linnaeus, 1767 (spelled as Linné), currently classified either in the genus *Astraea* [Röding], 1798 (type species *Trochus imperialis* Gmelin, 1791 by subsequent designation by Suter (1913, p. 166), Opinion 479 (1957)), or in the genus *Bolma* Risso, 1826, type species *Turbo rugosus* Linnaeus, 1767 by monotypy (spelled as 'Linné'). This means that *Cochlea* Garsault, 1764 is a senior subjective synonym either of *Astraea* [Röding], 1798, or of *Bolma* Risso, 1826, or of both, depending on the classification applied.

If *Astraea* [Röding], 1798 or *Bolma* Risso, 1826 are to remain in stable use they should be protected under Article 23.9 of the Code against *Cochlea* Garsault, 1764, or *Cochlea* should be suppressed for the purposes of the Principle of Priority, but not for those of the Principle of Homonymy. It is possible that the name *Cochlea* has been used somewhere since 1899. Since two generic names are currently used for *Turbo rugosus*, *Bolma* might be considered as not being in prevailing usage. An alternative would be to protect the use of *Astraea* by proposing to suppress *Cochlea* in those cases where *Trochus imperialis* Gmelin, 1791 and *Turbo rugosus* Linnaeus, 1767 (spelled as Linné) are classified in the same genus. For such a measure Article 23.9 has no provisions.

***Cochlea coelata* Garsault, 1764, pl. 644 (Gastropoda)**

Original spelling *Cochlea Caelata*. Text volume: Garsault, 1765, p. 376, there mentioned in the combination *Limax cochlea cœlata* ('*cochlea*' should be considered as a subgenus there). In the index (Garsault, 1764, p. 17) misspelled as *Cochlea celata*. Type locality Mediterranean Sea. Garsault's figure represents the marine snail *Turbo rugosus* Linnaeus, 1767 (spelled as Linné), currently classified in the genus *Astraea* [Röding], 1798. *Cochlea coelata* Garsault, 1764 is a senior synonym of *Turbo rugosus* Linnaeus, 1767 (spelled as Linné).

If the specific name *Turbo rugosus* Linnaeus, 1767 (spelled as Linné) is to remain in stable use for this species, it should be protected under Article 23.9 against *Cochlea coelata* Garsault, 1764. This should be effective as it seems unlikely that the name *Cochlea coelata* was used after 1899.

***Concha* Garsault, 1764, pl. 646 (Bivalvia)**

Text volume: Garsault, 1765, p. 377, there mentioned in the form *Ostreum concha margaritifera*, where ‘concha’ should be considered as a subgenus. Type species *Mytilus margaritiferus* Linnaeus, 1758 by monotypy, currently classified in the genus *Pinctada* [Röding], 1798, the type species of which is *Mytilus margaritiferus* Linnaeus, 1758 by subsequent designation (not researched). *Concha* Garsault, 1764 is a senior objective synonym of *Pinctada* [Röding], 1798.

If *Pinctada* [Röding], 1798 is to remain in stable use for this genus, *Concha* Garsault, 1764 needs to be suppressed for the purposes of the Principle of Priority, but not for those of the Principle of Homonymy. *Pinctada* could also be protected under Article 23.9, but it is possible that the name *Concha* has been used somewhere since 1899.

***Coturnix* Garsault, 1764, pl. 686 (Sauropsida, Aves)**

Appeared without species. Text volume: Garsault, 1765, p. 427, there misspelled as *Cothurnix*. Garsault’s figure represents the common quail *Tetrao coturnix* Linnaeus, 1758, currently classified in the genus *Coturnix* Bonnaterre, 1790. *Coturnix* Bonnaterre, 1790 was ruled by the Commission under the plenary power to be correctly established as a new name and available when *Coturnix* Brisson, 1760 was suppressed, type species *Tetrao coturnix* Linnaeus, 1758 ‘by absolute tautonymy’ according to Opinion 67 (1916), Directions 21 (1955) and 43 (1956). It seems inappropriate to us that the type species for a genus that was not available under the Code but made so by the Commission under the plenary power could be designated otherwise than under the plenary power.

Absolute tautonymy is not possible because *Tetrao coturnix* was not mentioned by Bonnaterre (1790, p. 217–218, verified in the 1823 edition, where the name *Coturnix communis* was listed among other names and a reference to Linnaeus, 1767 (spelled as Linné) (1766, p. 278) where *Tetrao coturnix* was listed). *Coturnix* Brisson, 1760 was established on p. 247 and suppressed for the purposes of the Principles of Priority and Homonymy in Direction 21 (1955), today unnecessary since this part of Brisson’s (1760) work was rejected for zoological nomenclature anyway (Directions 16 (1955), 105 (1963)). In this sense it is no longer necessary to consider *Coturnix* Bonnaterre, 1790 as having been made available by the Commission under the plenary power. We consider *Tetrao coturnix* as having been fixed as type species under the plenary power.

Since Garsault’s figure is in the sense of Bonnaterre (1790) and the rulings of the Commission, *Coturnix* Bonnaterre, 1790 must be shifted to Garsault’s (1764) authorship. Its type species is *Tetrao coturnix* Linnaeus, 1758 by designation under the plenary power. Authorship and year of *Coturnix* Bonnaterre, 1790 should be corrected on the Official List of Generic Names in Zoology.

***Ichthyocolla* Garsault, 1764, pl. 660 (Actinopterygii, Acipenseriformes)**

Appeared without species. Text volume: Garsault, 1765, p. 399. Listed by Sherborn (1902, p. 477), attributed to E.L. Geoffroy. In the Catalog of Fishes (online version 19 September 2008, entry *Huso*) the name was attributed to Geoffroy Saint-Hilaire, 1767. Garsault’s description referred to the beluga fish, *Acipenser huso* Linnaeus, 1758, currently classified by some authors in the genus *Ichthyocolla* as described by

Garsault (1764) (type species *Acipenser huso* Linnaeus, 1758, possibly designated by subsequent monotypy, Catalog of Fishes online version January 2004), by others in the genus *Huso* Brandt & Ratzeburg, 1833 (type species *Acipenser huso* Linnaeus, 1758 by absolute tautonymy) or in the genus *Acipenser* Linnaeus, 1758 (type species *Acipenser sturio* Linnaeus, 1758 by Linnean tautonymy). Garsault's (1764) figure does not represent the beluga fish, but seems to be either a fantasy object or composed of different species. The depicted actinopterygian fish has two distinct dorsal fins (the beluga has only one), an anal fin but no anal soft rays (the beluga has both), a smooth continuous and lipped mouth like a labrid (the beluga has a triangular snout with the apex attenuated to a point, turning slightly upward, with its lower lip interrupted at the centre), and no signs of scutes on the dorsal body, the typical feature of Acipenseriformes, so the figure cannot represent the beluga fish. Although the description from 1765 described the beluga, this species cannot be taken as the type species for the fish depicted in 1764 as *Ichthyocolla*, as Article 12.2.7 rules that the illustration must illustrate the taxon being named. The French name on the plate 'Grand Esturgeon Poisson à Colle' must be considered as a vernacular name and not as a description or definition which could make the name available. *Ichthyocolla* Garsault, 1764 remains unavailable for the beluga and could only be used for the figured actinopterygian fish, if this was a real species. The use of *Ichthyocolla* by Garsault (1765) was clearly a subsequent use of the 1764 name because it referred to the plate, so the 1765 description of the beluga has no relevance.

Pálsyni (1789) mentioned a name 'ichthyocolla' in an article about Acipenseriformes, but this name can hardly be interpreted as a generic name in the Linnean sense (for another Icelandic article on Acipenseriformes see Müller (1791) in the same journal, where the species is mentioned as *Acipenser huso*). *Huso* Brandt & Ratzeburg, 1833 is the correct generic name for *Acipenser huso* Linnaeus, 1758 if placed in a genus separate from *Acipenser sturio* Linnaeus, 1758.

***Lacerta terrestris* Garsault, 1764, pl. 668 (Sauropsida, Squamata)**

Established as *Lacertus terrestris*, *Lacertus* was an incorrect subsequent spelling for *Lacerta* Linnaeus, 1758. Text volume: Garsault, 1765, p. 412. Garsault's figure represents the wall lizard, *Seps muralis* Laurenti, 1768, currently classified as *Podacris muralis* (Laurenti, 1768). *Lacerta terrestris* Garsault, 1764 is a senior synonym of *Seps muralis* Laurenti, 1768.

If *Podacris muralis* is to remain in stable use for the wall lizard, the name should be protected under Article 23.9 against *Lacerta terrestris* Garsault, 1764.

***Leo* Garsault, 1764, pl. 716 (Mammalia)**

Appeared without species. Text volume: Garsault, 1765, p. 458. Garsault's figure represents the lion, *Felis leo* Linnaeus, 1758, currently classified in the genus *Panthera* Oken 1816 (type species is the leopard *Felis pardus* Linnaeus, 1758 by designation under the plenary power, Opinion 1368 (1985), where *Panthera* Oken, 1816 was ruled by the Commission to be available despite having been published in a non-binominal work). *Leo* Oken, 1816 is unavailable (published in a rejected non-binominal work, Opinion 417 (1956)), and *Leo* Brehm, 1829 is currently used as a subgenus (type species probably not yet designated, the two candidate species *Leo africanus* Brehm, 1829 and *Leo asiaticus* Brehm, 1829 are both junior synonyms of *Felis leo* Linnaeus,

1758). Both names are in the sense of Garsault's figure, so Brehm's (1829, p. 637) name has to be considered as a subsequent use of *Leo* Garsault, 1764 (it has no influence on this nomenclatural relationship that Brehm might have known the name from Oken or unpublished sources rather than from Garsault, 1764).

In the genus *Panthera* Oken, 1816 are currently classified the leopard *Panthera pardus*, the lion *Panthera leo*, the tiger *Panthera tigris* (Linnaeus, 1758), the ounce or snow leopard *Panthera uncia* (Schreber, 1775), and the jaguar *Panthera onca* (Linnaeus, 1758). *Leo* Garsault, 1764 would thus supersede *Panthera* Oken, 1816 as the earliest available generic name for this group of species. The names of the involved animals would have to be modified to *Leo pardus*, *Leo leo*, *Leo tigris*, *Leo uncia* and *Leo onca*.

If *Panthera* is to remain in stable use for this genus, and *Leo* is to remain available for a subgeneric name, then *Leo* Garsault, 1764 needs to be suppressed for the purposes of the Principles of Priority and of Homonymy.

***Ranetta* Garsault, 1764, pl. 672 (Amphibia)**

Appeared without species. Text volume: Garsault, 1765, p. 414. Listed by Sherborn (1902, p. 820), attributed to E.L. Geoffroy. Garsault's figure represents the tree frog *Rana arborea* Linnaeus, 1758, currently classified in the genus *Hyla* Laurenti, 1768 (type species *Hyla viridis* Laurenti, 1768 by subsequent designation by Stejneger 1907, p. 75; *Hyla viridis* Laurenti, 1768 is a junior synonym of *Rana arborea* Linnaeus, 1758). Apparently the genus *Ranetta* Garsault, 1764 has never been used and probably no specific name has ever been attached to it. Any other type species than *Rana arborea* Linnaeus, 1758 or one of its synonyms would be in contrast to Garsault's (1764) figure. So, if a type species were designated for *Ranetta* Garsault, 1764, this genus would become a senior synonym of *Hyla* Laurenti, 1768. *Hyla* should be conserved under Article 23.9, *Ranetta* has never been used.

***Rupicapra* Garsault, 1764, pl. 704 (Mammalia)**

Appeared without species. Text volume: Garsault, 1765, p. 447. Garsault's figure represents the chamois which was reported from the Alps, *Capra rupicapra* Linnaeus, 1758, currently classified in the genus *Rupicapra* Blainville, 1816 (type species is *Capra rupicapra* Linnaeus, 1758 by absolute tautonymy, confirmed in Opinion 91 (1926)).

The name *Rupicapra* must be shifted to Garsault's (1764) authorship, type species is *Capra rupicapra* Linnaeus, 1758 by absolute tautonymy by Blainville (1816, p. 75), or by subsequent designation, or as fixed under the plenary power in Opinion 91. *Rupicapra* as used by Blainville (1816, p. 75) must be considered as a subsequent use of *Rupicapra* Garsault, 1764. The authorship and year of *Rupicapra* on the Official List of Generic Names in Zoology, given as Blainville, 1816, is incorrect.

***Serpens* Garsault, 1764, pl. 667 (Sauropsida, Squamata)**

Appeared without species. Text volume: Garsault, 1765, p. 411. Garsault's figure represents the ring snake, *Coluber natrix* Linnaeus, 1758, currently classified in the genus *Natrix* Laurenti, 1768 (type species *Natrix vulgaris* Laurenti, 1768 by subsequent designation, not researched). The type species of *Natrix* Laurenti, 1768 is not *Coluber natrix* Linnaeus, 1758 by absolute tautonymy, since this name was not

originally included. Laurenti (1768) avoided tautonymies and established substitute names without mentioning the Linnean specific names.

The name *Serpens* has never been used. If a type species is attached to this genus, any species other than *Coluber natrix* Linnaeus, 1758 or one of its synonyms would not be in the sense of Garsault's (1764) figure. This means that once a type species is attached to *Serpens* Garsault, 1764, this will be the oldest available generic name for the group of species currently classified in the genus *Natrix* Laurenti, 1768. *Serpens* could eventually replace *Natrix*, but under Article 23.9 *Natrix* could be protected.

***Testudo terrestris* Garsault, 1764, pl. 675 (Sauropsida, Squamata)**

Text volume: Garsault, 1765, p. 417. Garsault's figure represents the European pond turtle (or tortoise, or terrapin), *Testudo orbicularis* Linnaeus, 1758, currently classified in the genus *Emys* Duméril, 1806. *Testudo terrestris* Garsault, 1764 is a junior synonym of *Testudo orbicularis* Linnaeus, 1758.

Testudo terrestris Garsault, 1764 is also a senior homonym of *Testudo terrestris* Forskål, 1775, which is currently used as a name for a subspecies of the Greek or spur-thighed tortoise *Testudo graeca* Linnaeus, 1758 in the combination *Testudo graeca terrestris* Forskål, 1775. In the course of rejecting names established by Fermin (1765), a non-binominal work which included a non-binominal name, *Testudo terrestris* Forskål, 1775 was ruled to be available by the Commission under the plenary power despite being a senior homonym of Fermin's (1765) name (Opinion 660 (1963)). We do not see a need for this name to be made available by the Commission under the plenary power despite being a senior homonym of Fermin's (1765) name if Fermin's (1765) work is rejected anyway and the name was not used until 1775.

If *Testudo graeca terrestris* is to remain in stable use for this subspecies, then *Testudo terrestris* Garsault, 1764 needs to be suppressed for the purposes of the Principles of Priority and of Homonymy.

***Trutta* Garsault, 1764, pl. 665 (Actinopterygii)**

Appeared without species. Text volume: Garsault, 1765, p. 409. Listed by Sherborn (1902, p. 1002), attributed to E.L. Geoffroy. Bogutskaya & Naseka (2004, p. 156) attributed the name to E.F. Geoffroy and the date 1764. Garsault's figure represents the trout, *Salmo trutta* Linnaeus, 1758 (original and current combination; the type species of *Salmo* Linnaeus, 1758 is the Atlantic salmon *Salmo salar* Linnaeus, 1758 by subsequent designation by Jordan & Gilbert, 1883, p. 309, Opinion 77 (1922) and Direction 56 (1956), also possible is Desmarest, 1856, p. 312, not researched). According to the Catalog of Fishes (online version January 2004), the type species of the genus *Trutta* is presumably *Salmo trutta* Linnaeus, 1758 by subsequent monotypy or subsequent designation or possibly absolute tautonymy. If *Salmo trutta* and *Salmo salar* are classified in different subgenera, a generic name for the group containing *Salmo trutta* is needed. *Trutta* Garsault, 1764 is available for this purpose.

***Turdus minor* Garsault, 1764, pl. 697 (Sauropsida, Aves)**

Text volume: Garsault, 1765, p. 440. Garsault's figure represents Swainson's thrush, *Turdus ustulatus* Nuttall, 1840, currently classified in the genus *Catharus* Bonaparte, 1850. *Turdus minor* Garsault, 1764 is a senior synonym of *Turdus ustulatus* Nuttall, 1840.

If *Catharus ustulatus* (Nuttall, 1840) is to remain in stable use for Swainson's thrush, it should be protected under Article 23.9 of the Code. We are not aware of a name *minor* having been used in the genus *Turdus*.

***Turtur* Garsault, 1764, pl. 685 (Sauropsida, Aves)**

Appeared without species. Text volume: Garsault, 1765, p. 427. Not *Turtur* Boddaert, 1783. Garsault's figure represents the Eurasian turtle-dove, *Columba turtur* Linnaeus, 1758, currently classified in the genus *Streptopelia* Bonaparte, 1855 (type species is *Columba risoria* Linnaeus, 1758, fixation not researched). This genus currently contains some twenty species (www.zoonomen.net, 2004). The type species of the genus *Turtur* Boddaert, 1783 is *Columba afra* Linnaeus, 1767 (spelled as Linné) (fixation not researched, data taken in 2004 from www.zoonomen.net). *Turtur* Boddaert, 1783 is currently in use. *Turtur* Garsault, 1764 must be considered as different and independent from *Turtur* Boddaert, 1783, resulting in *Turtur* Garsault, 1764 being a senior homonym of *Turtur* Boddaert, 1783. If a type species were designated for *Turtur* Garsault, 1764, this could only be *Columba turtur* Linnaeus, 1758 or one of its synonyms. In this case *Turtur* Garsault, 1764 would become a senior subjective synonym of *Streptopelia* Bonaparte, 1855 according to the current classification.

If *Turtur* Boddaert, 1783 is to remain in stable use for this bird genus, *Turtur* Garsault, 1764 needs to be suppressed for the purposes of the Principles of Priority and of Homonymy. If only *Streptopelia* is to remain in stable use for this genus, it should be protected by suppressing *Turtur* Garsault, 1764 for the purposes of the Principle of Priority, but not for those of the Principle of Homonymy.

***Vulpes* Garsault, 1764, pl. 702 (Mammalia)**

Appeared without species. Text volume: Garsault, 1765, p. 445. Garsault's figure represents the red fox, *Canis vulpes* Linnaeus, 1758, currently classified in the genus *Vulpes* Frisch, 1775 (type species *Canis vulpes* Linnaeus, 1758 fixed under the plenary power, Opinion 1129 (1982)). There are currently ten species classified in the genus *Vulpes*, the red fox being the only European species (Nowak, 1999, p. 636). Garsault (1764) used the name *Vulpes* in the same sense as Frisch (1775, interpreted in Opinion 1129 under the plenary power).

The name *Vulpes* must be shifted to Garsault's (1764) authorship (type species *Canis vulpes* Linnaeus, 1758 fixed under the plenary power). *Vulpes* as used by Frisch (1775, p. 15) must be considered as a subsequent use of *Vulpes* Garsault, 1764. Authorship and year of *Vulpes* on the Official List of Generic Names in Zoology, given as Frisch, 1775, is incorrect.

General nomenclatural evaluation

Four generic names have currently been used in combination with Geoffroy's erroneous 1767 authorship: *Ichthyocolla*, *Lucius*, *Merlangius* and *Trutta*. These names must be shifted to Garsault, 1764 without any further consequences. *Ichthyocolla* however, has to be considered as a nomen dubium, and *Lucius* has no relevance as a junior objective synonym of *Esox* Linnaeus, 1758.

Sixteen names established by Garsault (1764) should be used, including *Merlangius* and *Trutta*. This applies to the names *Alosa* Garsault, 1764 (hitherto known as *Alosa*

Linck, 1790), *Anguilla* Garsault, 1764 (hitherto known as *Anguilla* Schrank, 1798), *Asinus* Garsault, 1764 (up to now Gray, 1824), *Bufo* Garsault, 1764 (up to now Laurenti, 1768), *Castor canadensis* Garsault, 1764 (up to now Kuhl, 1820), *Coturnix* Garsault, 1764 (up to now Bonnaterre, 1790), *Cygnus* Garsault, 1764 (up to now Bechstein, 1803), *Martes* Garsault, 1764 (up to now Pinel, 1792), *Rupicapra* Garsault, 1764 (up to now Blainville, 1816), *Salamandra* Garsault, 1764 (up to now Laurenti, 1768), *Scincus* Garsault, 1764 (up to now Laurenti, 1768), *Tinca* Garsault, 1764 (up to now Cuvier, 1816), *Vipera* Garsault, 1764 (up to now Laurenti, 1768) and *Vulpes* Garsault, 1764 (up to now Frisch, 1775). Accordingly, *Antalis* Adams & Adams, 1854 should be attributed to Herrmannsen, 1846.

In addition to these names Garsault established eleven junior synonyms of genera that had already been established by Linnaeus (1758). These were *Antalium* Garsault, 1764 (of *Dentalium* Linnaeus, 1758), *Cetus* (of *Physeter*), *Cynos* (of *Hippopotamus*), *Harengus* (of *Clupea*), *Lucius* (of *Esox*), *Lupus* (of *Canis*), *Manati* (of *Trichechus*), *Merula* (of *Turdus*), *Monoceros* (of *Monodon*) and *Trutta* (of *Salmo*). The cod fish *Asellus* Garsault, 1764 is a junior homonym of the crustacean *Asellus* Geoffroy, 1762, and a junior subjective synonym of *Gadus* Linnaeus, 1758.

Rulings from the Commission are needed if the following nine names should remain in current usage: *Alces* Gray, 1821, *Gazella* Blainville, 1816, *Astraea* [Röding], 1798, *Pinctada* [Röding], 1798, *Panthera* Oken, 1816, *Potamon fluviatilis* (Herbst, 1785), *Testudo graeca terrestris* Forskål, 1775, *Turtur* Boddaert, 1783, *Streptopelia* Bonaparte, 1855.

As long as Garsault's senior synonyms have not been used, Article 23.9 of the Code can be applied to maintain stable use of *Astraea rugosa* (Linnaeus, 1767), *Capra aegagrus* Erxleben, 1777, *Catharus ustulatus* (Nuttall, 1840), *Podacris muralis* (Laurenti, 1768), *Hyla* Laurenti, 1768 and *Natrix* Laurenti, 1768.

Since Garsault's (1764, 1765, 1767) zoological works meet the requirements of the Code and are consistently binominal, it would not be justified to officially reject them, and we would not recommend such an action. If rejected, then at least *Merlangius* and *Trutta* would have to be ruled by the Commission under the plenary power to be available despite having been published in a rejected work. We recommend suppression of only those names that would threaten the stability of currently widely used names.

Lastly, the dramatic impact of the rediscovery of this work involving many well-known and important zoological names emphasises the need to cite, if possible, the author of a work on any occasion (names of taxa, bibliographical references) in the form it was printed on the title page of the corresponding work. The use of standardised names of zoological authors and initials of first names is very problematic and should be avoided. 'E.L.' was incorrectly added to Geoffroy by authors who did not verify the true identity and only assumed that E.L. Geoffroy was meant – others did not know this but relied for more than a century on this information. If only 'Geoffroy' had been cited, zoologists would have searched with much greater zeal for works published by authors with this name, and might have found E.F. Geoffroy and the true source, Garsault, earlier. In one case in a fish database which works semiautomatically with self-made standardised names of authors, Geoffroy was finally replaced by Geoffroy Saint-Hilaire, because only Geoffroy Saint-Hilaire was known as an author for fishes. Any deviation from the

original spelling of the author increases the distance from the original source of information and can finally lead to a complete disconnection. Garsault's example has shown that this can create serious problems even after more than 200 years.

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Case 3485***Lychnorhiza lucerna* Haeckel, 1880 (Cnidaria, Scyphozoa, Rhizostomeae): proposed conservation of generic and specific names**

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Abstract. The purpose of this application, under Article 23.9.3 of the Code, is to conserve the generic name *Lychnorhiza* Haeckel, 1880 for a genus of scyphozoan jellyfish (Cnidaria, Scyphozoa) from Central and South America and the Malayan Archipelago. The name is threatened by a senior synonym, *Rhacopilus* Agassiz, 1862, unused since its original description. It is also proposed to conserve the specific name *Lychnorhiza lucerna* Haeckel, 1880 (type species of the genus *Lychnorhiza*). The nomenclatural stability of this name is threatened by a senior subjective synonym, *Rhizostoma cruciata* Lesson, 1830.

Keywords. Nomenclature; taxonomy; Cnidaria; Scyphozoa; Rhizostomeae; LYCHNORHIZIDAE; *Lychnorhiza*; *Rhizostoma*; *Lychnorhiza lucerna*; *Rhizostoma cruciata*; scyphozoan jellyfish; Central and South America; Malayan Archipelago.

1. Lesson (1830, pp. 121–122) introduced the nominal species *Rhizostoma cruciata* based on specimens from the Brazilian coast (Santa Catarina State, Southern Region). The description does not mention the arrangement of the gastrovascular canals and thus could refer to any member of the group Inscapulatae, proposed by Stiasny in 1921. So far as can be ascertained, the type material studied by Lesson is lost or was never deposited in any collection (J. Goy, Institut Océanographique Paris, pers. comm.). Lesson (1843, p. 419) reiterated the original description.

2. Agassiz (1862, pp. 152–153) had access to an unpublished manuscript by J.P. Couthouy describing *Rhizostoma cyanolobata* from Rio de Janeiro Harbour, Brazil, and referred this species and *Rhizostoma cruciata* Lesson to the new genus *Rhacopilus* Agassiz, 1862 (p. 152), as *Rhacopilus cyanolobatus* Agassiz, 1862 (no type specimens are known to exist) and *Rhacopilus cruciatus*. Here, acting as First Reviser (Article 24.2 of the Code), I designate *Rhizostoma cruciata* Lesson, 1830 as the type species of the genus *Rhacopilus* Agassiz, 1862.

3. Haeckel (1880) proposed the new family LYCHNORHIZIDAE (p. 586), to accommodate the monotypic genus *Lychnorhiza* Haeckel, 1880 (p. 587) and the species *Lychnorhiza lucerna* Haeckel, 1880 (pp. 587–588) based on a single specimen (Zoological Museum Berlin, ZMB CNI 1170) from Rio de Janeiro, Southeastern Region, Brazil. In the same monograph, he established the genus *Cramborhiza* Haeckel, 1880 (p. 633), to accommodate the species *Cramborhiza flagellata* Haeckel, 1880 (p. 646) (Zoological Museum of the University of Copenhagen, ZMUC unnumbered), also found on the Brazilian coast in Pernambuco State,

Northeastern Region. In the same monograph the author commented on similarities in the morphology of the two species. Haeckel (1880, p. 620), using only the original description and information available in Agassiz (1862), included the species *Rhacopilus cruciatus* (Lesson, 1830) in the genus *Crambessa* Haeckel, 1869 (type species *Crambessa tagi* Haeckel, 1869, now *Catostylus tagi*, for which no type material exists) stating that *Rhacopilus cyanolobatus* Agassiz, 1862 and *Rhizostoma cruciata* Lesson, 1830 were conspecific. The name *cyanolobatus* Agassiz, 1862 has not been used as valid since 1899 (which meets the conditions of Article 23.9.1.1 of the Code), and the conditions of Article 23.9.1.2 are also met for *Lychnorhiza lucerna*; thus it is possible to apply Article 23.9.2 of the Code and declare the name *cyanolobatus* Agassiz, 1862 a nomen nudum.

4. Vanhöffen (1888, pp. 29–30) synonymised *Cramborhiza* with *Lychnorhiza* after having access to additional material from the Brazilian coast, but regarded *Cramborhiza flagellata* Haeckel, 1880 as distinct from *Lychnorhiza lucerna*. He commented on great similarity of *Cramborhiza flagellata* to *Lychnorhiza lucerna*, but mentioned that he could not solve the problem owing to inaccessibility of the type specimen of *Lychnorhiza lucerna*.

5. Mayer (1910, pp. 673–674), based on Vanhöffen's data, synonymised *Cramborhiza flagellata* under *Lychnorhiza lucerna*, arguing that the differences mentioned by previous authors were merely based on different developmental stages. Mayer (1910) referred the species *Rhizostoma cruciata* to the genus *Catostylus* Agassiz, 1862 (type species *Cephea mosaica* Quoy & Gaimard, 1824, now *Catostylus mosaicus*, neotype specimen G16878 in the Australian Museum).

6. Stiasny (1923, pp. 235–238), with the type specimens of *Cramborhiza flagellata* and *Lychnorhiza lucerna* at hand, agreed with Mayer that the two species were conspecific.

7. Vannucci (1951, pp. 94–95) described specimens of *Lychnorhiza lucerna* from the south and southeastern coasts of Brazil, and she also considered *Cramborhiza flagellata* and *Lychnorhiza lucerna* to be conspecific.

8. Kramp (1955, p. 167) re-examined the type specimen of *Cramborhiza flagellata* Haeckel deposited in the Copenhagen Museum (ZMUC unnumbered) and also considered that *Cramborhiza flagellata* and *Lychnorhiza lucerna* were conspecific.

9. Kramp (1961, pp. 366–367), in his synopsis of the medusae of the world, presented brief diagnoses and synonymies of the three *Lychnorhiza* species described up to that date: *Lychnorhiza arubae* Stiasny, 1920, *Lychnorhiza lucerna* Haeckel, 1880 and *Lychnorhiza malayensis* Stiasny, 1920. Kramp (1961, p. 370) also listed *Catostylus cruciatus* (Lesson, 1830).

10. Goy (1979, p. 292), while studying specimens from the Western South Atlantic Ocean (off Uruguay coast), identified five specimens as *Catostylus cruciatus*.

11. Mianzan & Cornelius (1999, p. 545) considered the species *Catostylus cruciatus* and *Lychnorhiza lucerna* conspecific, but were in some doubt about the synonymy because they did not examine the type specimens. Silveira & Cornelius (2000, p. 11, Tab. I) considered the record of *Catostylus cruciatus* doubtful. To confirm the identifications and synonymy the type specimens of *Cramborhiza flagellata* (ZMUC unnumbered), *Lychnorhiza lucerna* (ZMB CNI 1170), and the only available specimens identified as *Catostylus cruciatus* (deposited at the Muséum National d'Histoire Naturelle, MNHN Inv. M. 1719) were examined. The conclusion is that all

specimens belong to the same species. This resulted in a revision of the genus *Lychnorhiza* Haeckel, 1880 (Morandini, in prep.). This application is submitted to coincide as closely as possible with the publication of this revision.

12. The combination *Catostylus cruciatus* (Lesson, 1830) has been used only eight times in the literature, mainly in lists. Only Lesson (1830) and Goy (1979) mentioned collected specimens under this combination. In contrast, the name *Lychnorhiza lucerna* Haeckel, 1880 has been used more than four times more in literature (33 papers up to July 2009) and in more recent papers (Mianzan & Cornelius, 1999; Silveira & Cornelius, 2000; Schiariti et al., 2008).

13. In order to conserve the widely used generic name *Lychnorhiza* Haeckel, 1880, I propose that the name *Rhacopilus* Agassiz, 1862 is suppressed under Article 23.9.2 of the Code, as this name has not been used as valid since 1899 (Article 23.9.1.1). Regrettably, the junior synonym (*Lychnorhiza* Haeckel, 1880) does not meet all the requirements of Article 23.9.1.2, and therefore a ruling by the Commission is necessary to maintain the uniform use of *Lychnorhiza*. If the Principle of Priority is strictly followed, then the change must be performed, and it will generate confusion regarding the usage of the generic name as the junior synonym (*Lychnorhiza*) is widely used in research other than systematics (see more below).

14. In order to maintain stability in the nomenclature of this group I propose that the name *Lychnorhiza lucerna* Haeckel, 1880 be conserved by suppression of *Rhizostoma cruciata* Lesson, 1830. Again, strict adherence to the Principle of Priority will fail to recognise the most used name (an old problem in nomenclature – see support for the maintenance of the most familiar name in Howden et al., 1968 and Cornelius, 1987). Repercussion of the rejection of the most used junior synonym (*Lychnorhiza lucerna*) will cause much confusion in literature, because the name *Lychnorhiza lucerna* is extensively applied in papers on ecology, faunal lists, reproduction, life cycle, and acoustic characterisation. The species is one of the commonest jellyfishes along the Argentinean and Brazilian coasts (Morandini et al., 2005; Schiariti et al., 2008), and is used regularly as material for practical zoology courses, especially in Brazil (Haddad, 2006, pp. 42–43).

15. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to suppress the following names for the purposes of the Principle of Priority, but not for those of the Principle of Homonymy:
 - (a) *Rhacopilus* Agassiz, 1862;
 - (b) *cruciata* Lesson, 1830, as published in the binomen *Rhizostoma cruciata*, and all the uses of this name before 1880;
- (2) to place on the Official List of Generic Names in Zoology the name *Lychnorhiza* Haeckel, 1880 (gender: feminine), type species by original designation *Lychnorhiza lucerna* Haeckel, 1880;
- (3) to place on the Official List of Specific Names in Zoology the name *lucerna* Haeckel, 1880, as published in the binomen *Lychnorhiza lucerna* and as defined by the holotype ZMB CNI 1170 in the Zoological Museum Berlin (specific name of the type species of *Lychnorhiza* Haeckel, 1880);
- (4) to place on the Official Index of Rejected and Invalid Generic Names in Zoology the name *Rhacopilus* Agassiz, 1862 as suppressed in (1)(a) above;

- (5) to place on the Official Index of Rejected and Invalid Specific Names in Zoology the name *cruciata* Lesson, 1830, as published in the binomen *Rhizostoma cruciata* and as suppressed in (1)(b) above.

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Comments on this case are invited for publication (subject to editing) in the *Bulletin*; they should be sent to the Executive Secretary, I.C.Z.N., c/o Natural History Museum, Cromwell Road, London SW7 5BD, U.K. (e-mail: iczn@nhm.ac.uk).

Case 3494***Atlanta inflata* Gray, 1850 (Mollusca, Gastropoda, PTEROTRACHEOIDEA, ATLANTIDAE): proposed conservation of the specific name**

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Abstract. The purpose of this application, under Article 23.9.5 of the Code, is to conserve the specific name of the heteropod *Atlanta inflata* Gray, 1850 (ATLANTIDAE), originally published as a primary homonym of the pteropod *Atlanta inflata* d'Orbigny, 1836 (usually cited as *Limacina inflata*, currently *Heliconoides inflata*) (LIMACINIDAE). Both names are in use and have not been considered congeneric since the 19th century. We propose to conserve the name *Atlanta inflata* Gray, 1850 by ruling that the name is not invalid by reason of being a junior primary homonym.

Keywords. Nomenclature; taxonomy; Gastropoda; Heteropoda; Pteropoda; PTEROTRACHEOIDEA; ATLANTIDAE; LIMACINIDAE; *Atlanta*; *Limacina*; *Atlanta inflata*; *Heliconoides inflata*; gastropods.

1. The genus *Atlanta* was described by Lesueur (1817, p. 390) and included two species. The type species (de Blainville, 1825, p. 493, subsequent designation) is *A. peronii* Lesueur, 1817 (p. 390), a holoplanktonic gastropod of the family ATLANTIDAE Rang, 1829 (superfamily PTEROTRACHEOIDEA Rafinesque, 1814, Heteropoda Lamarck, 1812) (Bouchet & Rocroi, 2005, pp. 88, 251).

2. The pteropod species *Atlanta inflata* d'Orbigny, 1836 is currently assigned to the genus *Heliconoides* d'Orbigny, 1836 (see Janssen, 2003, p. 168), but for a long time was considered to be a species of the genus *Limacina* (Thecosomata, LIMACINIDAE). Gray (1850b, p. 31) and Boas (1886, p. 48) were the first to transfer *Atlanta inflata* to the genus *Limacina*, followed by almost all later authors, although some used other, synonymous names, such as *Spirialis* or *Spiratella* (compare van der Spoel, 1967, p. 50; Janssen & Zorn, 2001, p. 148).

3. The name of the heteropod *Atlanta inflata* was formally introduced by Gray (1850a), who latinised the French vernacular name 'Atlante renflée', as published by Eydoux & Souleyet (1841?) and later by Souleyet (1852), under whose authorship the taxon is usually cited. Ever since its introduction, *Atlanta inflata* Gray, 1850 has been considered to belong to the genus *Atlanta* (e.g. Tesch, 1906, pp. 8, 55; van der Spoel, 1976, p. 149 (and many references therein); Richter, 1987, p. 179; Seapy, 1990, p. 123; Richter & Seapy, 1999, p. 636).

Some authors (Thiriot-Quiévreux, 1969, p. 354; Abbott, 1974, p. 133) used the name *Atlanta quoyii* Gray, 1850 (or its junior synonym *A. quoyana* Souleyet, 1852) instead of *A. inflata*. That taxon was considered to be a probable synonym of *A. inflata* by Tesch (1908, p. 20), which was acknowledged by Richter (1974, p. 62), who, acting as First Reviser (Article 24.2.1 of the Code), fixed precedence of the name *Atlanta inflata*. Unlike *A. inflata*, no syntypical material of *A. quoyii* remains.

4. Both names, *Atlanta inflata* d'Orbigny, 1836 (currently *Heliconoides inflata*) and *Atlanta inflata* Gray, 1850, have been in common use for over a hundred years, and the latter has never been referred to another genus (van der Spoel, 1976, p. 149). Introducing a replacement name for the well-known name *Atlanta inflata* Gray, 1850 offers nothing but loss of information and confusion. This application for the conservation of prevailing usage is submitted in the interest of stability and in accordance with Article 23.9.5 of the Code (names not considered congeneric after 1899).

5. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to rule that the specific name *Atlanta inflata* Gray, 1850 is not invalid by reason of being a junior primary homonym of *Atlanta inflata* d'Orbigny, 1836;
- (2) to place on the Official List of Specific Names in Zoology the following names:
 - (a) *inflata* Gray, 1850, as published in the binomen *Atlanta inflata*, with the endorsement that it is not invalid by reason of being a junior primary homonym of *Atlanta inflata* d'Orbigny, 1836;
 - (b) *inflata* d'Orbigny, 1836, as published in the binomen *Atlanta inflata*.

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Case 3488

***Papilio danae* Fabricius, 1775 (currently *Colotis danae*; Insecta, Lepidoptera, PIERIDAE): proposed conservation of prevailing usage by the suppression of *Papilio danae* Hufnagel, 1766**

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Abstract. The purpose of this application, under Article 23.9.5 of the Code, is to conserve the name *Papilio danae* Fabricius, 1775 (Lepidoptera, PIERIDAE) which, as *Colotis danae*, is well-established as the valid name for a common and widespread butterfly with many subspecies in circa 50 countries in Africa, Arabia, and Asia. The name is threatened by the primary homonym *Papilio danae* Hufnagel, 1766. It is proposed that Hufnagel's name be suppressed for the purposes of the Principle of Homonymy under the plenary power of the Commission, in the interest of nomenclatural stability.

Keywords. Nomenclature; taxonomy; Insecta; Lepidoptera; PIERIDAE; *Papilio*; *Colotis*; *Callosune*; *Colotis danae*; *Papilio eborea*; butterflies; Asia; Arabia; Africa.

Present status

1. *Colotis danae* (Fabricius, 1775, p. 476) (Lepidoptera, PIERIDAE Swainson, 1820) is now generally recognised as the nominate Indian subspecies of the beautiful – and unmistakable – ‘Scarlet [Crimson] Tip’. It was, as were all species of butterflies (PAPILIONOIDEA Latreille, 1802) named by Fabricius in 1775, described in the genus *Papilio* (the type locality was listed as ‘India orientali. Mus. tottianum’; two syntypes

are in the Zoological Museum, University of Copenhagen). Most other species now placed in the genus *Colotis* Hübner, 1819, were first described in a variety of genera, chiefly *Teracolus* Swainson, 1833 and *Callosune* Doubleday, 1847; *Papilio danae* was designated as type species of the latter by Scudder (1875). Afrotropical species of *Colotis* were also occasionally described in the genera *Papilio* Linnaeus, 1758, *Pieris* Schrank, 1801, *Pontia* Fabricius, 1807, *Euchloe* Hübner, 1819, *Anthocharis* Boisduval, Rambur & Graslin, 1833, *Idmais* Boisduval, 1836, *Anthopsyche* Wallengren, 1857, *Thestias* Boisduval, 1836, or *Madais* Swinhoe, 1909.

2. Though established in 1819 the generic name *Colotis* remained largely unused, apparently mainly because its type species was not understood. Hübner (1819, p. 97) had included seven species in the genus, five of which now belong in *Colias* Fabricius, 1807, the type genus of the subfamily COLIADINAE Swainson, 1821, while only two were true *Colotis* (as currently understood), belonging to the PIERINAE. Scudder (1875, pp. 146–147) designated *Papilio amata* Fabricius, 1775 as the type species but, owing to confusion with *C. calais* Cramer, 1775 (and its subsequent redescription by Stoll, 1781), it was considered that the genus *Colotis* was based on a misidentification and *Colotis* remained unused for another 25 years (see Hemming, 1967, for a summary of this complex issue).

3. Kirby (1871) did not mention *Colotis* at all, but Kirby (1896) did use the name *Colotis* as valid, though he retained *danae* in the genus *Callosune* Doubleday, 1847, of which it was the type species. Bingham (1907), in a major book in the ‘Fauna of British India’ series, was the first to use the name *Colotis* for all the Indian species. In a brief paper, Talbot (1931) accepted that *Colotis* should be used as the valid name for species formerly included in *Teracolus* Swainson, 1833 (and by implication *Callosune*) with the words: ‘The genus *Colotis* was obviously meant by Hübner to characterise species well known to all later authors as *Colias*. It was unfortunate that a totally different insect was selected by Scudder as the type. The name is thus restricted and *Teracolus* must sink’. In his influential identification guide to all Indian butterflies, Evans (1932) followed Bingham in considering *Colotis* the senior synonym of all other generic names previously used.

4. Talbot (1939a) revised the PIERIDAE in the ‘Fauna of British India’, now using *Colotis* for all the Indian species, reflecting his view in *Lepidopterorum Catalogus* (Talbot, 1934). In the same year, Talbot (1939b) revised the species-level taxonomy of the African *Colotis*. This established (i) the use of *Colotis* also for all African taxa, as well as (ii) the use of *C. danae* for the current African subspecies that had previously been applied at the species level (*Thestias annae* Wallengren, 1857, *Pontia eupompe* Klug, 1829, *Teracolus pseudacaste* Butler, 1876, and *Teracolus walkeri* Butler, 1884).

5. At present, the combination *Colotis danae* (see para. 10 for an exception) is almost unanimously considered the valid specific name of this species in about 50 countries in Africa, Arabia and India (a list of more than 38 cases of usage of the combination between 1907 and 2007 is held by the Commission Secretariat).

Homonymy

6. Koçak (1981) correctly pointed out that the Indian *Papilio danae* Fabricius, 1775 is a junior primary homonym of the European *Papilio danae* Hufnagel, 1766 (p. 82). He considered the latter to be a valid name and therefore proposed that the valid

name for the species currently known as *C. danae* should be *Papilio eborea* Stoll, 1781 (Stoll, 1781, pp. 120–121, 249, pl. 352, figs. C–F). Stoll was actually continuing the work of Cramer (1775), and Cramer is sometimes erroneously cited as the author of *eborea*.

6. *Papilio danae* Hufnagel, 1766 was described in a very obscure journal and the type is presumed lost. On the few occasions it has been mentioned, it was considered to be a synonym of *Papilio semele* Linnaeus, 1758, currently known as *Hipparchia semele* (SATYRINAE Boisduval, 1833), a widespread European nymphalid butterfly (e.g. Kudrna, 1977). The name has only been mentioned occasionally, even in synonymy, since the early 19th century, and probably ever since its description in 1766. It has not been used as a valid species-group name for 200 years or more (except by Koçak and his associates, see para. 10).

7. The combination *Papilio danae* was also proposed by Cramer (1775) for a Neotropical species of NYMPHALIDAE from ‘Surinam’. However, the Commission (Opinion 516, Opinions and Declarations 19: 1–44, 16 May 1958) ruled that names published by Cramer (1775) should be deemed to have been published on 31 December and they are therefore junior to those described by Fabricius (1775). *Papilio danae* Cramer, 1775 is currently known as *Historis odius dious* Lamas, 1995 (in Lamas et al., 1995 (NYMPHALIDAE, COEINI Scudder, 1893 – Neotropical)). *P. danae* Cramer is thus a junior homonym, and it is not relevant to the present issue.

Conservation of *Papilio danae* Fabricius, 1775 as a valid name

8. In the authoritative catalogue on African butterflies (Ackery et al., 1995) the authors stated, with reference to Koçak (1981): ‘The name *Colotis danae* Fabricius has been in widespread use since its establishment. We propose to make an application to the I.C.Z.N. to here set aside the principle of priority in order to maintain stability by conserving *Colotis danae* Fabricius as a valid taxon’. Such an application has not yet been made.

9. The name *danae* has been unequivocally used for the nominate Indian species, now placed in *Colotis*, since 1775. The combination *Colotis danae*, in various subspecies, has consistently been used for the Indian population since 1907, and for the African populations since the 1940s. Several thousand publications include references to the combination *Colotis danae*, and the combination ‘*Colotis danae*’ yields about 1,700 references (‘hits’) using the internet search engine ‘Google’. In contrast ‘*Papilio danae* Hufnagel’ and ‘*Hipparchia danae* Hufnagel’ yield 12 hits in all, none as a valid name except for references to its original description.

10. While there is no doubt that *Papilio eborea* Stoll, 1781 is a redescription of *Papilio danae* Fabricius, it is rarely mentioned in the literature (e.g. Kirby, 1896) and then mainly as a junior synonym of *Colotis danae*. Chainey (2005) states that no type material has been located, but that it represents a valid species (with reference to Bridges, 1988, who accepted the view of Koçak, 1981). However, only in recent publications by Koçak and his collaborators (e.g. Kemal, 2004; Koçak & Kemal, 2007; and in various computer-generated lists) is *eborea* actually being used as the valid name. It would be a barely understood replacement name for the well-known *Colotis danae*. The combination ‘*Colotis eborea*’ receives four hits in ‘Google’, while ‘*Callosune eborea*’ and ‘*Teracolus eborea*’ yield none. The combination ‘*Papilio*

eborea Cramer' receives two hits. Most of these simply treat the name in summary lists that also contain *Colotis danae*, or with the caution that the status of the name is uncertain. The current draft list of names of world butterflies of the family PIERIDAE under the Taxome Project does include the combination *Colotis eborea* as the valid name by Lamas (2008), but this has not yet been formally published. G. Lamas (pers. comm.) informed us that in view of its history and usage he would prefer to see *Colotis danae* conserved. Consultations with other lepidopterists show support for the application since it was first mooted by Ackery et al. (1995).

11. *Colotis danae* has been used as an indicator species in many studies: e.g. biogeographical (Bernardi, 1989; Larsen, 1984, 1987); ecological (Fitzherbert et al., 2006; Gardiner, 2004; Larsen, 1987/1988); and molecular (Nazari, in prep.). Such studies are of wider interest because the species is represented in the Indian subcontinent as well as in Africa, increasing the desirability of nomenclatural stability. The species has been bred on numerous occasions and the early stages are figured in several of the papers listed in this application, and in the additional list of references held by the Secretariat. From such documentation, as well as from earlier papers, the name *Colotis danae* (and, by inference, its original combination *Papilio danae*) has also permeated into botanical, general, and popular natural history literature.

12. It would serve no useful purpose to change the established usage of *Colotis danae* (Fabricius, 1775) to maintain a primary homonym (*Papilio danae* Hufnagel, 1766) that has hardly appeared in print since its publication. Its adoption would cause unnecessary confusion, not least since the combination *Colotis danae* is currently used in 50 or more countries in two major biogeographical regions, with several currently recognised subspecies, and at least 25 other names that are either placed in synonymy or infrasubspecific. The fact that its potential replacement name, *Papilio eborea*, has rarely been mentioned, and then mainly as a junior synonym or in name lists, would only compound the confusion.

13. Since this issue concerns a genuine case of primary homonymy raised in 1981 and 1995, and since *Colotis danae* has recently been treated as invalid by Koçak and collaborators, it can only be resolved through a ruling by the Commission in order to maintain nomenclatural stability and to reduce potential future confusion.

14. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to suppress the name *danae* Hufnagel, 1766, as published in the binomen *Papilio danae* and all uses of the name before that by Fabricius, 1775, for the purposes of both the Principle of Priority and the Principle of Homonymy;
- (2) to place on the Official List of Specific Names in Zoology the name *danae* Fabricius, 1775, as published in the binomen *Papilio danae*;
- (3) to place on the Official Index of Rejected and Invalid Specific Names in Zoology the name *danae* Hufnagel, 1766, as published in the binomen *Papilio danae* and as suppressed in (1) above.

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Case 3492***Heliconius tristero* Brower, 1996 and *Heliconius melpomene mocoa* Brower, 1996 (Lepidoptera, NYMPHALIDAE): proposed conservation by suppression of *Heliconius melpomene bellula* Brown, 1979**

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Abstract. The purpose of this application, under Articles 23.9.3 and 81.2.3 of the Code, is to conserve the species-group names *Heliconius tristero* Brower, 1996 and *Heliconius melpomene mocoa* Brower, 1996 (Lepidoptera: NYMPHALIDAE) for mimetic butterflies from the Putumayo region of southeastern Colombia by suppressing the senior name *Heliconius melpomene bellula* Brown, 1979. The authorship of this name is convoluted, *bellula* having been originally proposed as an unavailable quadri-nomen and made available by Brown (1979). More significantly, because the implied holotype of *Heliconius melpomene bellula* Brown, 1979 does not belong taxonomically to the species whose oldest available name is *Heliconius melpomene* (Linnaeus, 1764), *H. melpomene bellula* Brown, 1979 is not a subjective synonym of *H. melpomene* (Linnaeus, 1764). Strict application of the Code would synonymise *H. tristero* Brower, 1996 with *H. bellula* Brown, 1979 and retain *H. melpomene mocoa* Brower, 1996 as a valid name. That action would reverse the current application of nomenclature for these taxa as published in numerous recent papers and result in significant confusion. Furthermore, given current disagreement among authors as to the taxonomic status of the specimen associated with the description of the name *bellula*, future nomenclatural instability is likely if the name is maintained. It is therefore proposed that the name *Heliconius tristero* be conserved and the name *Heliconius bellula* be suppressed.

Keywords. Nomenclature; taxonomy; Insecta; Lepidoptera; NYMPHALIDAE; *Heliconius*; *Heliconius tristero*; *Heliconius melpomene mocoa*; *Heliconius melpomene bellula*; butterflies; Colombia.

1. Stichel (1923) described a single specimen of *Heliconius amaryllis amaryllis* C. & R. Felder, 1862, with the name *Heliconius amaryllis amaryllis* Forma *bellula* from the vicinity of Mocoa in the upper Putumayo region of southeastern Colombia. *Heliconius amaryllis* is currently viewed as a geographical race or subspecies of *Heliconius melpomene* (Linnaeus, 1758) (cf. Ackery & Smiles, 1976; Lamas, 2004), occurring in the Huallaga Valley of northeastern Peru (Brown, 1979; Sheppard et al., 1985; Mallet & Barton, 1989). As a quadri-nomen, the name *Heliconius amaryllis amaryllis* Forma *bellula* Stichel, 1923 is infrasubspecific and unavailable (Articles 10.2, 45.5 of the Code – Infrasubspecific names), and cannot be made available from its original publication by the regulations in any other Article of the Code, except by

a Commission's ruling (Article 45.5.1 of the Code). The provisions of Article 45.6 are not relevant in this case as they deal only with names following a binomen, while '*bellula*' is following a trinomen.

2. Lamas (1998) asserted that under Articles 10c, 23j, 50c of the 3rd Edition of the Code (1985), Turner (1971) made available and became author of the name *Heliconius melpomene bellula*, based on his listing of the name '*bellula* (?)' as a geographical race of *Heliconius melpomene* in a figure legend. However, Turner (1971) did not mention the name *bellula* anywhere else in the book chapter, and his listing of the name in the figure legend provided neither a description of the taxon (Article 13.ai), nor citation of a previous author's description (or even indication of Stichel as the author) (Article 13a.ii). Therefore, Turner's use of the name does not satisfy the criteria of availability stated in Article 13.1 (13a of the 1985 Code), and *Heliconius melpomene bellula* Turner, 1971 is a nomen nudum.

3. Brown (1979, Appendix 3, p. 117) made available and became the author of the name *Heliconius melpomene bellula* by treating it as a subspecies of *Heliconius melpomene* and citing Stichel (1923) (Articles 10.2, 13.1.2, 45.5.1, 50.3.1 of the Code – Authorship of an infrasubspecific name), which by implication made Stichel's original specimen the holotype of the new nominal taxon (Articles 72.5.6, 73.1.2 of the Code – Eligibility as name-bearing types). Subsequent authors (e.g. Sheppard et al., 1985; Mallet, 1993; Holzinger & Holzinger, 1994; Brower, 1996b) applied the name to the geographical race of *H. melpomene* from southeastern Colombia, and followed Brown (1979) in attributing authorship to Stichel (1923). The specimen described by Stichel (1923) became the holotype of *H. melpomene bellula* Brown, 1979 by monotypy (Article 73.1.2).

4. Brower (1996a) discovered based on mitochondrial DNA sequences that the then-current taxonomic concept of '*Heliconius melpomene bellula*' comprised two taxa – a subspecies of *H. melpomene* and a relative of the clade comprising *Heliconius cydno* Doubleday, 1847, *H. heurippa* Hewitson, [1854] and *H. timareta* Hewitson, 1867. Brower described and illustrated the former as *H. melpomene mocoa* and the latter as *Heliconius tristero*, and also examined and illustrated the holotype of Brown's *bellula* (= Stichel's single specimen). There is some suggestion that *H. tristero* was among the first species to be discovered and diagnosed by a combination of morphological features and DNA sequences. Brower pointed out two salient features of the Stichel specimen: that its genitalia imply it is related to the *cydno* group of species, and that its wing-pattern markings suggest that it is a hybrid backcross between *H. tristero* and another member of the *cydno* group. This interpretation is supported by the observation that Stichel (1923) first diagnosed the 'typical' form of *H. amaryllis amaryllis* Felder and then described Forma *bellula* as a variant of it with yellow spots on the forewings (see Brower, 2000).

5. Lamas (1998) published a critique of Brower's (1996a) taxonomic determinations, citing the Glossary of the 1985 Code 'the progeny of two individuals belonging to different subspecies of the same species are not hybrids') to argue that the name *bellula* could not be denied validity under Article 23h on the basis of its putative hybrid origin. Lamas synonymised *H. melpomene mocoa* Brower, 1996 with *H. melpomene bellula*, the species he attributed to Turner, 1971. Lamas repeated this synonymy in his widely-used checklist of Neotropical butterflies (Lamas, 2004) (see 2 and 3 above for a revised view on authorship and typification).

6. Brower (2000) published a response to Lamas (1998), again pointing out that Stichel's specimen, the holotype of *H. melpomene bellula* Brown, 1979, was not taxonomically an example of *H. melpomene*. Thus, Lamas' (1998) synonymy was incorrect. Lamas (1998) rejected Brower's (1996a) hypothesis of a hybrid origin of Stichel's specimen because no other relatives of *H. tristero* were then known from the vicinity of Mocoa. However, there is now evidence for additional cryptic *cydno* group taxa masquerading as other *melpomene* races in adjacent regions both north and south of Mocoa (Giraldo et al., 2008; Jiggins et al., unpublished data) that could provide alleles resulting in the putative hybrid wing-pattern elements observed in Stichel's specimen of *bellula*. Brower (2000) presented formal synonymies of the names *mocoa* and *bellula* and also addressed the taxonomic status of the *bellula* specimen as a 'hybrid'. Of course, given its age, the morphological attributes of this specimen cannot be corroborated by evidence from DNA, as have the taxonomic identities of the types of the names described by Brower (1996a). Note that although the name *bellula* Brown, 1979 would be excluded from zoological nomenclature under Article 1.3.3 if its holotype were deemed taxonomically to be a hybrid, the argument presented in this petition assumes that the name is available.

7. Subsequent authors (Brower & Egan, 1997; Mallet et al., 1998, 2007; Shaw, 1998; Penz, 1999; Brower, 2002, 2006; Gilbert, 2003; Lamas, 2004; Bull et al., 2006; Beltrán et al., 2007; Kronforst et al., 2007; Giraldo et al., 2008; Jiggins, 2008; Mallet, 2009) as well as numerous websites have universally employed the name *H. tristero* Brower, 1996 to refer to the relative of the *cydno* group, and *H. melpomene bellula* Turner, 1971 or *H. melpomene mocoa* Brower, 1996 for the subspecies of *H. melpomene*. This species complex is the subject of intense research due to the hypothesis of 'hybrid speciation' (cf. Mávarez et al., 2006), and the patterns of relationships among the taxa are already confused enough without a needless nomenclatural muddle. A list of 28 published and electronic examples of usage is held by the Commission Secretariat.

8. If the provisions of the Code are followed and the name *bellula* is available, then *Heliconius tristero* Brower, 1996 is a junior subjective synonym of *Heliconius bellula* Brown, 1979 (which is not taxonomically a member of the species *Heliconius melpomene*), while the name *Heliconius melpomene mocoa* Brower, 1996 is the only available name for the entity traditionally called *H. melpomene bellula*. This reversal of current usage is not satisfactory for the reasons indicated above.

9. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to suppress the name *bellula* Brown, 1979, as published in the trinomen *Heliconius melpomene bellula*, for the purposes of the Principle of Priority but not for those of the Principle of Homonymy;
- (2) to place on the Official List of Specific Names in Zoology the following names:
 - (a) *tristero* Brower, 1996, as published in the binomen *Heliconius tristero*;
 - (b) *mocoa* Brower, 1996, as published in the trinomen *Heliconius melpomene mocoa*;
- (3) to place on the Official Index of Rejected and Invalid Specific Names in Zoology the name *bellula* Brown, 1979, as published in the trinomen *Heliconius melpomene bellula* (senior subjective synonym of *Heliconius tristero* Brower, 1996) and as suppressed in (1) above.

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Case 3487***Megalosaurus crenatissimus* Depéret, 1896 (currently *Majungasaurus crenatissimus*; Dinosauria, Theropoda): proposed replacement of the holotype by a neotype**

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Abstract. The purpose of this application, under Articles 75.5 and 75.6 of the Code, is to replace the existing, non-diagnostic holotype of *Megalosaurus crenatissimus* Depéret, 1896 (currently *Majungasaurus crenatissimus*) by a neotype. The designation of a neotype is necessary to conserve the prevailing usage and concept of the species.

Keywords. Nomenclature; taxonomy; Reptilia; Archosauria; Dinosauria; Theropoda; ABELISAUROIDAE; *Majungasaurus*; *Megalosaurus*; *Megalosaurus crenatissimus*; *Majungatholus atopus*; Madagascar; Cretaceous.

1. Charles Depéret (1896, p. 188) named the theropod dinosaur species *Megalosaurus crenatissimus* based on isolated and fragmentary fossils from the Upper Cretaceous Maevarano Formation near Berivotra, Mahajanga Province, Madagascar. These materials had been collected in 1895 by French military personnel, and were eventually repositied in the collections of the Faculté des Sciences (FSL) at the Université Claude Bernard, Lyon (Krause et al., 2007). Although Depéret did not explicitly designate any one of these specimens as the holotype, they nonetheless collectively represent the name-bearing type specimens for the species *crenatissimus*.

2. René Lavocat (1955, p. 259) erected a new genus *Majungasaurus* for the theropod species *crenatissimus*, based on a new specimen (Muséum National d'Histoire Naturelle specimen MNHN.MAJ 1) that he designated as the neotype. This material consisted of an incomplete right dentary that came from the same stratum and general area as the specimens described by Depéret (Article 75.3.6 of the Code), and which was sufficient to distinguish the species from other members of the genus *Megalosaurus*.

3. Subsequently, the putative pachycephalosaurian dinosaur *Majungatholus atopus* Sues & Taquet, 1979 was named, based on a partial skull roof from the Maevarano Formation in the Mahajanga area (MNHN.MAJ 4) (Sues & Taquet, 1979, p. 634).

4. Later discoveries of more complete specimens from the same stratum and area demonstrated that *Majungatholus atopus* was not a pachycephalosaur, but rather an unusual theropod belonging to the family ABELISAUURIDAE (Sampson et al., 1998).

5. Restudy of MNHN.MAJ 1 has confirmed that it is diagnostic, and belongs to the same taxon as the holotype braincase of *Majungatholus atopus*, making the latter a junior synonym of *Majungasaurus crenatissimus* (Krause et al., 2007, pp. 6–7; Sampson & Witmer, 2007, p. 85).

6. However, the original name-bearing type specimens of *Megalosaurus crenatissimus* were neither lost nor destroyed, and they remain in the collections of the Faculté des Science, Université Claude Bernard, Lyon (FSL specimens 92.289, 92.290, 92.306, 92.343). Lavocat's only justification for creating a neotype was the insufficiently diagnostic nature of the original name-bearing materials (Lavocat, 1955). This violates Article 75.3.4 of the Code and invalidates the neotype designation for *Majungasaurus crenatissimus*.

7. As several authors have confirmed (Sampson et al., 1996, 1998; Krause et al., 2007), the original Depéret type specimens are indeed indeterminate as to genus and species, although they can be identified as belonging to the family ABELISAUURIDAE. Thus the taxonomic identity of the species *Megalosaurus crenatissimus* as a nominal species-group taxon cannot be determined from the existing name-bearing type materials.

8. The theropod species *Megalosaurus crenatissimus* has been referenced for more than a century (e.g. Thevenin, 1907; Huene, 1926; Piveteau, 1926; Depéret & Savornin, 1928; Stromer, 1931; Steel, 1970; Russell et al., 1976; Molnar, 1990; Bonaparte, 1991; Sampson et al., 1996; Krause et al., 2007), and remains the only large theropod species from the Upper Cretaceous Maevarano Formation of Madagascar. The genus *Majungasaurus* has been in use continually since 1955 for this species (e.g. Lavocat, 1955; Huene, 1959; Romer, 1966; Steel, 1970; Molnar, 1980, 1990; Carroll, 1988; Bonaparte, 1991; Sampson et al., 1996; Krause et al., 2007), with all of the cited authors referring to MNHN.MAJ 1 as the neotype specimen. Thus the (incorrectly designated) neotype defines the 'accustomed meaning' of this 'long-accepted name' (Introduction to the Code).

9. Because the taxonomic identity of the nominal species *Megalosaurus crenatissimus* Depéret, 1896 cannot be determined from its existing name-bearing type series, the stability of the species and genus names, both long entrenched in the scientific literature, are threatened (Article 75.5 of the Code). The specimen MNHN.MAJ 1 is available and diagnostic (Krause et al., 2007; Sampson & Witmer, 2007), and would maintain prevailing usage both of the name *Majungasaurus crenatissimus* and of the

particular specimen as its neotype (Article 75.6 of the Code and Recommendation 75D). MNHN.MAJ 1 is well known to researchers and has been illustrated previously in publications (Lavocat, 1955, fig. 1; Krause et al., 2007, fig. 7). If a neotype is not designated, the name *Majungasaurus crenatissimus* will be restricted to the original type series, which would (a) not reflect the currently understood concept of this taxon, but also (b) effectively eliminate the name from functional use.

10. The lack of a neotype exacerbates an ongoing difficulty associated with understanding dinosaur diversity by preventing resolution of the question of synonymy between *Majungasaurus crenatissimus* and *Majungatholus atopus*. Without a neotype the two taxa cannot be synonymised with certainty, as has been recently suggested by Krause et al. (2007), because the holotype of *Megalosaurus crenatissimus* is indeterminate. Maintaining them as distinct taxa inadequately reflects present understanding of theropod diversity in the Maevarano Formation, and has led to the incorrect assessment of multiple large theropod taxa solely because of these taxonomic difficulties (e.g. Weishampel et al., 2004).

11. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to set aside all previous type fixations for the nominal species *crenatissimus* Depéret, 1896, as published in the binomen *Megalosaurus crenatissimus*, and to designate the specimen MNHN.MAJ 1 as the neotype;
- (2) to place on the Official List of Generic Names in Zoology the name *Majungasaurus* Lavocat, 1955 (gender: masculine), type species through designation by Lavocat (1955) *Megalosaurus crenatissimus* Depéret, 1896;
- (3) to place on the Official List of Specific Names in Zoology the name *crenatissimus* Depéret, 1896, as published in the binomen *Megalosaurus crenatissimus* and as defined by the neotype designated in (1) above (specific name of the type species of *Majungasaurus* Lavocat, 1955).

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Case 3479***Cuvieronius* Osborn, 1923 (Mammalia, Proboscidea): proposed conservation**

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Abstract. The purpose of this application, under Articles 68 and 75 of the Code, is to conserve the generic name *Cuvieronius* Osborn, 1923, long and widely applied to extinct South American gomphotheriid proboscideans, by setting aside all previous type species fixations and designating *Mastotherium hyodon* Fischer, 1814 as the type species of *Cuvieronius* and by designating a neotype for that species. Because *M. hyodon* has long and universal (though incorrect) usage as the type species of *Cuvieronius* and because the type specimen of *M. hyodon* is unidentifiable, the type species and neotype designations are proposed to promote the stability and universality of nomenclature.

Keywords. Nomenclature; taxonomy; Mammalia; Proboscidea; GOMPHOTHERIIDAE; Brazil; *Cuvieronius*; *Cordillerion*; *Haplomastodon*; *Mastotherium hyodon*; *Cuvieronius tarijensis*; South America; Ecuador; Chile; Argentina.

1. Cuvier (1806, p. 413) described the first gomphothere fossils from South America using the term ‘mastodonte des cordilières’ for an upper molar from Ecuador and ‘mastodonte humboldien [sic]’ for an incomplete lower molar or deciduous premolar from Chile. Cuvier (1824, p. 527) later introduced the Latinised names *Mastodon andium* and *Mastodon humboldtii* for these teeth.

2. Fischer (1814, p. 341) had already coined the binomen *Mastotherium hyodon* for the ‘mastodonte des cordilières’ of Cuvier (1806) and the binomen *Mastotherium humboldtii* for the ‘mastodonte humboldien’ [sic] of Cuvier (1806). Thus, the species name *M. hyodon*, as the first Code-compliant binomen proposed, has priority over *Mastodon andium* Cuvier, 1824.

3. In the nineteenth and early twentieth centuries, most palaeontologists envisioned two taxa of extinct South American proboscideans, one from the uplands (Andean Cordillera) and the other from the lowlands (especially the Argentine pampas and Brazilian coastal lowlands). The name *Mastodon andium* was applied to the Cordilleran species, whereas the name *Mastodon humboldtii* came to be applied to the lowland species (e.g. Nordenskiöld, 1903; Boule & Thevenin, 1920).

4. Osborn (1923, 1926) named two genera to match this distinction. He (1923, p. 1) named the lowland species *Cuvieronius* (type species *Mastotherium humboldtii* Fischer, 1814, by original designation, though Osborn erroneously attributed the species name to Cuvier, 1824) and (1926, p. 15) the upland species *Cordillerion* (type species *Mastodon andium* Cuvier, 1824, by original designation).

5. Beginning with Cabrera (1929), all workers agreed that the specimens described by Cuvier (1806), for which Osborn proposed two generic names, are teeth of one taxon (e.g. Hoffstetter, 1950, 1952; Simpson & Paula Couto, 1957; Ficarelli et al., 1995; Ferretti, 2008; Lucas, 2008).

6. Cabrera (1929, p. 69) considered *Cordillerion* Osborn, 1926 to be a junior subjective synonym of *Cuvieronius* Osborn, 1923, and all subsequent authors (except Osborn, 1936) have agreed with him on this (e.g. Simpson, 1945; Hoffstetter, 1950, 1952, 1955; Simpson & Paula Couto, 1955, 1957; Parodi Bustos, 1962; Tobien, 1973; Laurito, 1988; Casamiquela et al., 1996; Shoshani & Tassy, 1996; McKenna & Bell, 1997; Lambert & Shoshani, 1998; Frassinetti & Alberdi, 2000; Alberdi et al., 2002, 2004; Prado et al., 2002, 2003, 2005; Ferretti, 2008; Lucas, 2008). However, Cabrera (1929, pp. 80–82) applied the generic name *Cuvieronius* to the upland gomphothere, by erroneously naming *Mastotherium hyodon* as its type species, even though he acknowledged the original type species designations of Osborn (1923, 1926). Cabrera (1929, p. 81) did this because he regarded *Mastotherium hyodon* as a senior synonym of both *Mastodon andium* Cuvier, 1824 and *Mastodon humboldtii* (Fischer, 1814). Accordingly, he used the genus-group name *Cuvieronius*, stating *Mastotherium hyodon* to be its type species, even though he acknowledged the original type species designation of *Mastodon humboldtii* (Fischer, 1814) proposed by Osborn (1923).

7. Cabrera (1929) thus chose the oldest named species (*Mastotherium hyodon*) as the type species of *Cuvieronius* and thereby reassigned the designated type species of *Cordillerion* (*M. hyodon*) to the genus *Cuvieronius* as its type species, even though *M. humboldtii* was originally designated as the type species of *Cuvieronius*. Although this is a clear violation of Article 68.2 of the Code, subsequent authors accepted Cabrera's type species assignments (e.g. Hoffstetter, 1950, 1952; Simpson & Paula Couto, 1957; Parodi Bustos, 1962; Tobien, 1973; Laurito Mora, 1988; Lucas et al., 1997; Lambert & Shoshani, 1998; Lucas et al., 1999, 2000; Frassinetti & Alberdi, 2000; Montellano-Ballesteros, 2002; Prado et al., 2002, 2003, 2005; Ferretti, 2008; Lucas, 2008). The authoritative works of Hoffstetter (1952) and Simpson & Paula Couto (1957) may be considered particularly influential in gaining universal acceptance of this nomenclatural error.

8. The holotype tooth of *Mastotherium hyodon* is MNHN (Musée National d'Histoire Naturelle, Paris) AC 1738 and was photographically illustrated by Ficarelli et al. (1995, pl. 89, fig. 5). It is considered to have been collected from the Cangahua deposits near Imbabura Volcano in Ecuador. The holotype tooth of *Mastodon humboldtii* is MNHN AC 1743 and was collected in either Chile or Ecuador near Concepcion. These kinds of cheek teeth are very similar in *Cuvieronius* Osborn, 1923 and *Haplomastodon* Hoffstetter, 1950, so the holotype teeth of *M. hyodon* and of *M. humboldtii* are not identifiable to genus level (e.g. Simpson & Paula Couto, 1957; Ficarelli et al., 1995; Lucas, 2008). Indeed, palaeontologists working on South American proboscidean fossils have long regarded only one species of *Cuvieronius* as valid (*C. hyodon*) and one species of *Haplomastodon* as valid (*H. waringi*) and distinguish these species from each other by characteristics of the skull and tusks, not by the cheek teeth, which cannot be identified to species or even genus level when found isolated (see references cited under paragraph 7 above).

9. Ficarelli et al. (1993) documented new finds of *Haplomastodon* from Ecuador. Based on these fossils, Ficarelli et al. (1995) argued that the holotype upper second

molar of *Mastotherium hyodon*, which is from Ecuador, is likely a molar of *Haplomastodon*, simply because no fossils identifiable as *Cuvieronius* have been found in the Cangahua deposits of Ecuador, which is the likely source of the holotype tooth of *M. hyodon*.

10. Recognising the problematic taxonomic position of the holotype molar of *Mastotherium hyodon* (it cannot be identified as either *Cuvieronius* or *Haplomastodon*, and its provenance actually suggests it is the latter), Ficarelli et al. (1995, p. 753) proposed to 'revive the term *tarijensis* [a nomen nudum in Ameghino, 1902]' and to designate a specimen from the Tarija, Bolivia, gomphothere sample described originally by Boule & Thevenin (1920) as a holotype. They thus chose a skull and mandible (MNHN TAR 1270, originally illustrated by Boule & Thevenin, 1920, pls. 1–3) from Tarija as the holotype of a new species, *Cuvieronius tarijensis*, which they claimed was the first species of South American *Cuvieronius* based on an identifiable holotype. Ficarelli et al. (1995) thus recommended abandoning the name *C. hyodon* as a nomen dubium.

11. Ameghino (1902, p. 2), in a table, had listed *Mastodon tarijensis* as a taxon from Tarija, Bolivia, with no description given. Hoffstetter (1952, p. 187) correctly regarded this name as a nomen nudum, a point also stressed by Prado et al. (2003, 2005), who considered *Cuvieronius tarijensis* Ficarelli et al., 1995 (p. 747) a synonym of *Cuvieronius hyodon*. However, the fact that Ameghino (1902) introduced the species name *tarijensis* as a nomen nudum does not affect its availability, so Ficarelli et al. (1995) did name a new species of *Cuvieronius*, *C. tarijensis*.

12. The solution to the nomenclatural problems of *Cuvieronius* and its type species proposed by Ficarelli et al. (1995) upsets the stability and universality of nomenclature. It requires that a century of usage of *Cuvieronius hyodon* be abandoned by referring all South American *Cuvieronius* to *C. tarijensis*.

13. Instead, in accordance with Articles 68 and 75 of the Code, I propose a solution to the nomenclatural problems that surround *Cuvieronius* and its type species that promotes the stability and universality of nomenclature: designate *Mastotherium hyodon* Fischer, 1814 the type species of *Cuvieronius* and designate as neotype of *M. hyodon* the skull and lower jaw from Tarija, Bolivia, originally described and illustrated by Boule & Thevenin (1920, pls. 1–3): MNHN TAR 1270.

14. The International Commission on Zoological Nomenclature is accordingly asked:

(1) to use its plenary power:

(a) to set aside all previous type species fixations for *Cuvieronius* Osborn, 1923 and to designate *Mastotherium hyodon* Fischer, 1814 as the type species;

(b) to designate the skull and lower jaw from Tarija, Bolivia originally described and illustrated by Boule and Thevenin (1920, pls. 1–3): MNHN TAR 1270 as neotype of *Mastotherium hyodon*;

(2) to place on the Official List of Generic Names in Zoology the name *Cuvieronius* Osborn, 1923 (gender: masculine), type species *Mastotherium hyodon* Fischer, 1814, as ruled in (1)(a) above;

(3) to place on the Official List of Specific Names in Zoology the name *hyodon*, as published in the binomen *Mastotherium hyodon* Fischer, 1814 (specific name of the type species of *Cuvieronius* Osborn, 1923) and as defined by the neotype designated in (1)(b) above.

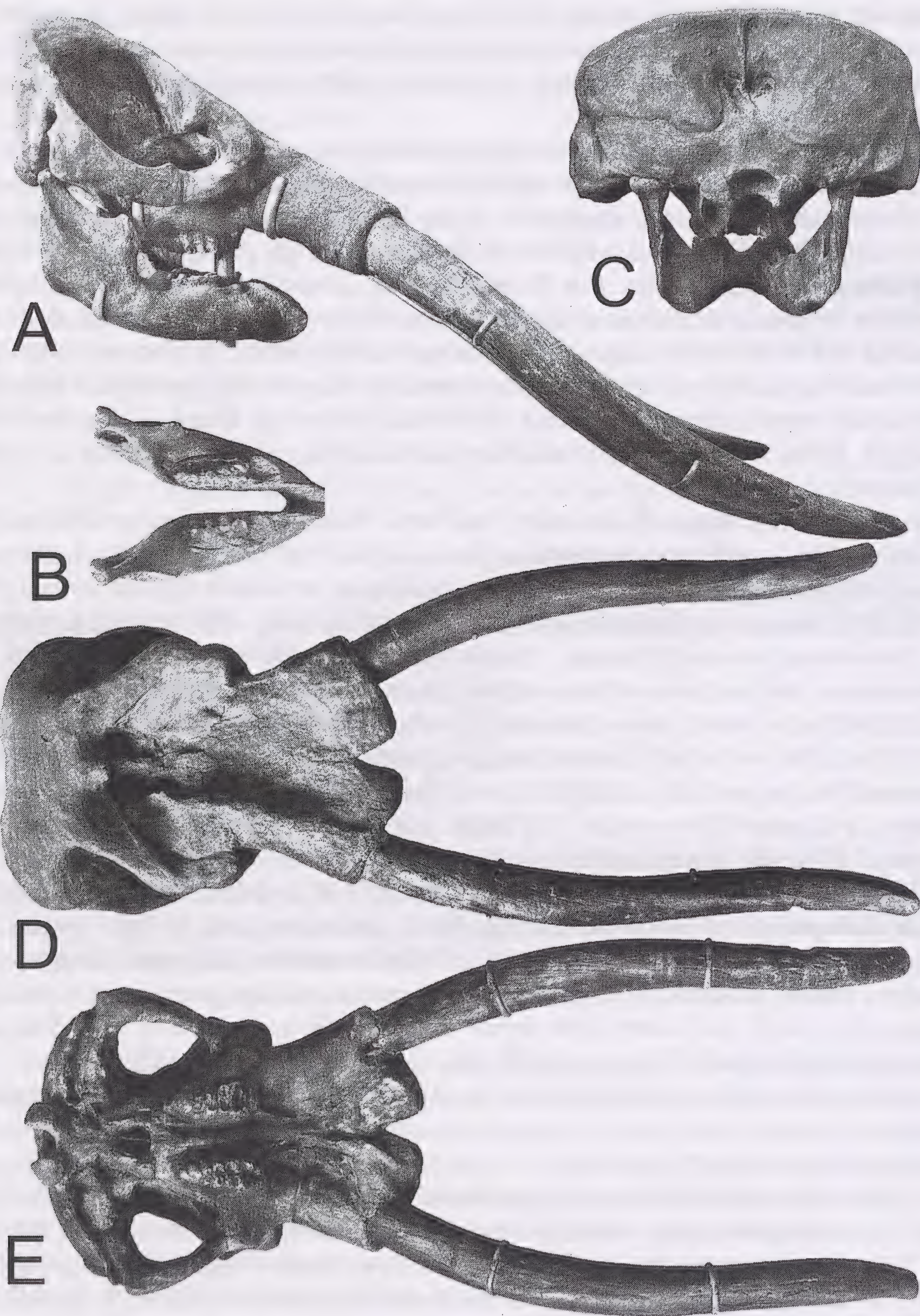


Fig. 1. MNHN (Musée National d'Histoire Naturelle, Paris) TAR 1270, skull and lower jaw from Tarija, Bolivia, proposed as neotype of *Mastotherium hyodon* Fischer, 1814. A, Right lateral view of skull and lower jaw. B, Occlusal view of lower jaw. C, Occipital view of skull and lower jaw. D, Dorsal view of skull. E, Ventral view of skull. For scale, maximum length of skull (including tusks) = 210 cm. Modified from Boule & Thevenin (1920, pls. 1–3).

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Acknowledgement of receipt of this application was published in BZN **65**: 242.

Comments on this case are invited for publication (subject to editing) in the *Bulletin*; they should be sent to the Executive Secretary, I.C.Z.N., c/o Natural History Museum, Cromwell Road, London SW7 5BD, U.K. (e-mail: iczn@nhm.ac.uk).

Comment on the proposed establishment of availability of *Balintus* d'Abrera, 2001, *Gulliveria* d'Abrera & Bálint, 2001, *Salazaria* d'Abrera & Bálint, 2001, *Megathecla* Robbins, 2002 and *Gullicaena* Bálint, 2002 (Insecta, Lepidoptera, LYCAENIDAE)
(Case 3458; see BZN 65: 188–193)

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In Case 3458 Robbins and Lamas ('the applicants') inform us (BZN 65: 189, para. 2) that a checklist of butterflies was being compiled (Lamas, 2004) which took more than 12 years to reach publication. Perhaps not surprisingly, within this period someone else published a book (d'Abrera, 2001) in which were proposed 8 new genera within the LYCAENIDAE, ascribed to d'Abrera or to d'Abrera & Bálint ('the authors'). The applicants at first considered most of these names to be available but later declared that all were unavailable owing to the phrasing of their respective diagnoses. This decision was made principally on the applicants' interpretation of the authors' meaning, based on words and phrases inserted into the authors' text by the applicants themselves. They claimed that the authors had 'purported to differentiate the type species but not the genus' in each case. Bálint and d'Abrera naturally disagreed with this assertion and an impasse was reached.

The bone of contention revolves around a minor and rather trivial perceived technicality. Taking the example cited by the applicants, under the heading 'Genus *Annamaria* d'Abrera & Balint gen. nov.' the authors (in d'Abrera, 2001) wrote '...However, is distinguished from *Evenus* by...' followed by several distinguishing characters of the wing veins and androchonial patches. This was clearly 'a definition that states in words characters that are purported to differentiate the taxon' in the sense of the Code (Article 13.1.1), in other words the authors' purpose in including this paragraph was to differentiate the new genus. How successfully it achieved this purpose and how comprehensive were the chosen characters is a matter of taxonomic opinion and not covered by the requirements of the Code. This is why the Code includes the term 'purported'. If the authors had instead written '...this genus is distinguished from *Evenus* by...' there could have been no doubt of its availability, but the applicants interpreted '...is distinguished...' as meaning '...the type species is distinguished...' and so concluded that the authors had not in fact distinguished the genus.

The monotypic genera *Gulliveria*, *Balintus*, *Lucilda*, *Pedusa*, and *Riojana* were differentiated by the authors using characters taken from the type species of each genus. Although denied by the applicants, the characters of the type species are the characters of their respective monotypic genera; there are no other characters the authors could have provided to differentiate these taxa. *Chopiniana* was described with two included species, both of which were distinguished from other genera by the authors, thus making the genus available from its description.

The generic diagnoses given do not appear to make any of these new genera taxonomically unsound; the applicants themselves use *Gulliveria* d'Abrera & Bálint, 2001 and *Annamaria* d'Abrera & Bálint, 2001 as valid (under the names of *Megathecla* Robbins, 2002 and *Lamasina* Robbins, 2002 respectively) and also *Salazaria* and *Balintus* (BZN 65: 191, para. 15); *Lucilda*, *Pedusa*, and *Riojana*, being

monotypic genera and thus unambiguous, are taxonomically sound if their genus-level rank is accepted. *Chopiniana* is presumably equally sound in the absence of any comments to the contrary. This suggests that it is not the taxonomic concepts that the applicants find ambiguous or unacceptable, but rather the names themselves and/or their authorship.

There is a flaw in the concept of *Megathecla* Robbins, 2002, proposed as a replacement name for *Gulliveria* d'Abrera & Bálint, 2001 (non Castelnau, 1878). If the applicants consider *Gulliveria* to be unavailable owing to an inadequate diagnosis, then *Megathecla* must also be unavailable as it rests on the same 'inadequate' diagnosis. At best *Megathecla* could be argued to be an unnecessary replacement name and a nomen nudum. Yet the Commission is being asked to reward this error of Robbins (2002) by making *Megathecla* available, while rejecting most of the new genera of d'Abrera & Bálint (in d'Abrera, 2001) as a punishment for their ambiguous phraseology.

Contrary to what the title of Case 3458 might suggest, the applicants are in fact asking the Commission (BZN 65: 192, clause 4) to officially reject seven of the new names proposed by d'Abrera and Bálint, two of which it will have just made available in the same application. The Commission should not consider rejecting some generic names that are inconvenient to the applicants while making others available. Such a ruling would engender ill-feeling and a sense of injustice and would exacerbate the nomenclatural confusion currently caused by this issue. The Commission would be rubberstamping various breaches of good zoological practice (see the Code, Appendix A, Code of Ethics) that have already occurred in the course of this matter. It would also set an unacceptable precedent that any taxonomist who might interpret the description of an earlier taxon as being inadequate, or as meaning something which conflicts with the Code, could correctly declare it unavailable and rename it her/himself.

But it is appropriate that the Commission should rule on the availability of the eight generic names proposed in d'Abrera (2001) as these are the source of all disagreement and subsequent nomenclatural confusion. The only just solution is to give the authors the benefit of the doubt, in that their diagnoses meant what they appeared to mean, and to rule that all eight of the new generic names in d'Abrera (2001) are available from their original descriptions. All other issues will then simply be settled by the conditions of the Code.

Comment on the proposed conservation of the specific name of *Chrysophanus florus* Edwards, 1884 (currently *Lycaena florus*) (Insecta, Lepidoptera, LYCAENIDAE) by designation of a neotype for *Polyommatus castro* Reakirt, 1866 (currently *Lycaena castro*)

(Case 3450; see BZN 66: 136–143)

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I support the application to conserve the name *Chrysophanus florus* Edwards, 1884 (currently *Lycaena florus*). My studies of the immature stages of *Lycaena florus*, as cited in the references of Case 3450, support the conclusion it is a different species from *Lycaena helloides* (Boisduval, 1852).

The uncertainty of *Lycaena castro* (Reakirt, 1866) is rectified by erecting a neotype for *L. castro* and making it synonymous with *L. helloides*.

Comment on the proposed precedence of *Chelodina rugosa* Ogilby, 1890 (currently *Macrochelodina rugosa*; Reptilia, Testudines) over *Chelodina oblonga* Gray, 1841 (Case 3351; see BZN 63: 187–193, 64: 68, 127–128, 65: 62, 66: 79)

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I write to support the proposal by S.A. Thomson to give precedence to the name *Chelodina rugosa* Ogilby, 1890 over *Chelodina oblonga* Gray, 1841, allowing the name *Chelodina collei* Gray, 1856 to be used appropriately (see also Fritz, 2008, BZN 65: 62) for the snakeskin turtle of southwestern Australia to which the name *C. oblonga* has been misapplied since 1967 (Thompson, 2000; Iverson et al., 2001; Thomson, 2006, BZN 63: 187–193).

Thompson's proposal 1) solves a complex nomenclatural problem with its roots in an unpublished 1967 thesis (Thomson, 2000; Thomson, 2006, BZN 63: 187–193; 2007, BZN 64: 127–128); 2) gives proper credit to early scientists and their published descriptions, diagnoses, and type designations (Thomson, 2007, BZN 64: 127–128); 3) minimises current name changes at the species (Kraus, 2009, BZN 66: 79) and genus levels (Iverson et al., 2001; Thomson, 2007, BZN 64: 127–128); and 4) anticipates future probable taxonomic designations (Georges, 2009, BZN 66: 79).

For these same reasons I do not support the alternative proposal of McCord & Joseph-Ouni (2007, *Reptilia* 52: 56–64) and Savage (2007, BZN 64: 68) to solve the problem by designating a neotype for *Chelodina oblonga*. That designation would unnecessarily complicate the taxonomy of western Australian snakeskins now and in the future.

Comments on the proposed conservation of usage of *Testudo gigantea* Schweigger, 1812 (currently *Geochelone (Aldabrachelys) gigantea*; Reptilia, Testudines)
(Case 3463; see BZN 66: 34–50, 80–87, 169–186)

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My colleagues and I (Takahashi et al., 2003) published a paper in which we made morphological and taxonomic comparisons of a fossil tortoise with other testudines. We have become aware that there has been great nomenclatural debate and uncertainty about the name of the Aldabra tortoise, and after studying the recent publication of Case 3463 in the Bulletin of Zoological Nomenclature, it is clear that the nomenclatural instability adds greatly to the complications of scientific research.

It is therefore very important to fix the name of the Aldabra tortoise, and the detailed explanation in Case 3463 shows why this should be based on the name that has been in use for more than a century. The maintenance of the neotype of *T. gigantea*, based on a specimen of known locality and exhibiting gigantism, a critical character of this species, will be the most satisfactory way to fix the name, and facilitate scientific investigation in various fields that need stable names.

Additional references

Takahashi, A., Hirayama, R. & Otsuka, H. 2003. A new species of the genus *Manouria* (Testudines: Testudinidae) from the Upper Pleistocene of the Ryukyu Islands, Japan. *Paleontological Research*, 7(3): 195–217.

(2) Ren Hirayama

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I completely support the petition to conserve the specific name *Testudo gigantea* Schweigger, 1812 for the Aldabra tortoise, although I used another name, *Dipsochelys dussumieri* in my paper (Takahashi et al., 2003). I agree with Frazier that *T. gigantea* is the established name. As explained in Case 3463, it has been in continuous use for more than 100 years and has been widely used in the scientific literature. It is important to settle this nomenclatural issue because there has been considerable confusion since 1982. The neotype for *T. gigantea* (USNM 269962) that was designated in 2006 should be maintained.

(3) Angel C. Alcala

Silliman University, Angelo King Center for Research and Environmental Management (SUAKCREM), 2/F Marine Lab. Bldg, Bantayan, 6200 Dumaguete City, Philippines (e-mail: suakcrem@yahoo.com)

I agree with the arguments of Dr Jack Frazier recommending the stabilisation of the name of the Aldabra tortoise and proposing that the neotype designation of 2006 be affirmed. Changing the scientific name of this tortoise will lead to confusion.

(4) Miguel A. Carretero

CIBIO, Centro de Investigação em Biodiversidade e Recursos Genéticos, Campus Agrário de Vairão, 4485–661 Vairão, Portugal (e-mail: carretero@mail.icav.up.pt)

As a student of reptiles in the Indian Ocean area I was perfectly aware of the nomenclatural chaos regarding the Aldabra tortoises having profound effects on both the research progress in this field and conservation efforts for preserving and managing these tortoises. I completely agree with Frazier's arguments and hopefully this will provide a stable taxonomical situation for the future.

(5) Igor G. Danilov

Department of Herpetology, Zoological Institute, Russian Academy of Sciences, Universitetskaya Nab. 1, St. Petersburg, 19903 Russia
(e-mail: dig@mail333.com & turtle@zin.ru)

My message is in support of Case 3463 to stabilise the name of the Aldabra tortoise. I do agree with arguments described in Case 3463. I think this is the only way to stop the considerable taxonomic and nomenclatural confusion that surrounds the Aldabra tortoise and stabilise its scientific name.

(6) Carl H. Ernst

Department of Vertebrate Zoology, Division of Amphibians and Reptiles, National Museum of Natural History, Smithsonian Institution, Washington, DC, 20013–7012 U.S.A. (e-mail: chernst@frontiernet.net)

I support the application of J. Frazier to conserve the name *Testudo gigantea* Schweigger, 1812 for the Aldabra tortoise by maintenance of the neotype designation, and suppression of *Testudo dussumieri* Gray, 1831. Frazier (2006a, 2006b) clearly described the recent confusion as to the acceptable name for the Aldabra tortoise following Bour's (1982) claim that the holotype of *T. gigantea* was not an Aldabra tortoise and his creation of the genus *Dipsochelys*. *Dipsochelys* Bour, 1982 has been used by some turtle systematists and hobbyists, but either *Geochelone* Fitzinger, 1835 or *Aldabrachelys* Loveridge & Williams, 1957 has been preferred by most biologists and the greater number of testudinologists. Very importantly, both of these names have precedence over *Dipsochelys*, and both of them are far more common in the literature. Likewise, *gigantea* is by far the most frequently used species name for the Aldabra tortoise, and pre-dates both *elephantina* Duméril and Bibron, 1835 and *dussumieri* Gray, 1831. The most recent checklist of the world's turtles (Fritz & Havaš, 2007) used *Aldabrachelys gigantea* for this tortoise. Frazier's (2006a) designation of a neotype (USNM 269962) fixed the name *Testudo gigantea* to the giant tortoises from Aldabra Atoll, thereby stabilising the nomenclature by using the established name. Bour (2007) supposedly rediscovered the type specimen of *Testudo gigantea* Schweigger, 1812, which was hard to do because Schweigger never listed one, and the holotype (by monotypy) has never been described a second time, and has been lost for nearly two centuries. However, the name of the Aldabra tortoise should be *Testudo gigantea* (currently *Geochelone* (*Aldabrachelys*) *gigantea* (Schweigger, 1812)) with a neotype designated by Frazier (2006a).

(7) R. Honegger

Zurich Zoological Garden, CH 8044 Zurich, Switzerland

As Curator of Herpetology (Retired) of Zurich Zoological Garden, Zurich, Switzerland, and a past Chairman of the Swiss Federal Scientific Commission to CITES, I support the petition by Frazier to conserve the specific name *gigantea* for the Aldabra giant tortoise. This charismatic tortoise species is well-known under this name and such a species deserves to have a stable name.

I am very glad that Dr Jack Frazier is now applying to conserve the specific name *gigantea*. Within the frame of our daily zoo work we do need stable names. Zoological Gardens within the World Association of Zoos and Aquaria (WAZA) are visited by more than 600 million visitors each year. During my professional life of over 40 years, I have visited about two hundred zoos in Europe, the U.S.A., Africa, Asia and Australia, and noticed that a majority of them label their Aldabra tortoises with the specific name *gigantea*. Zoos have an important duty in educating all people from young pupils to university graduate students, and will continue to refer to this species as *gigantea*. The new names proposed will not have the power to change this fact. International organisations such as IUCN or WWF also use the specific name *gigantea* for the Aldabra giant tortoise.

Another very important issue is the implementation of conservation regulations (such as CITES) at international border posts. As zoo curators we are regularly asked for assistance when animals or animal products are checked by customs. Furthermore we educate customs officers in regular training sessions for identifying animal species. How can we teach customs officers if such relevant questions of naming such important animals are not satisfyingly solved? Destabilising the nomenclature of this threatened species creates potential loopholes in laws and regulations concerning its protection. There will be confusion and chaos when important conservation regulations should be strictly implemented. I do hope very much that the ICZN will accept the arguments given in Case 3463 and give this fascinating tortoise the name it deserves: *gigantea*.

(8) Jeffrey E. Lovich

*USGS, Southwest Biological Science Center, 2255 N. Gemini Drive, MS-9394
Flagstaff, AZ 86001-1600 U.S.A. (e-mail: jeffrey_lovich@usgs.gov)*

I am writing in support of the application by Jack Frazier to conserve the name *Testudo gigantea* for the Aldabra tortoise and to recognise the neotype he designated. This species has one of the most confusing (unnecessarily so) taxonomies of any living turtle, despite the fact that it is internationally recognised and iconic. Given the multitude of names proposed for this species, common usage of *gigantea* begs for a resolution as suggested by Frazier. Cleaning up past confusion regarding names and types is the only way to achieve a solution to this long-standing problem.

(9) Patrick K. Malonza

Herpetology Section, National Museums of Kenya, Museum Hill Road, P.O. Box 40658–00100, Nairobi, Kenya

(e-mail: pkmalonza@yahoo.com & kmalonza@museums.or.ke)

This is to clarify that, although I am the author of a scientific paper that used the scientific name *Dipsochelys dussumieri* to refer to the Aldabra tortoise (Malonza, 2003), I have now read Case 3463 and I support the arguments explained therein. Not only is the claim of rediscovery of the holotype of *Testudo gigantea* Schweigger, 1812 not totally convincing but, more importantly, there is an urgent need to stabilise the name of the Aldabra tortoise using the established species name *gigantea*. This is especially important to make research and conservation activities on this endangered species more effective.

Additional references

Malonza, P.K. 2003. Ecology and distribution of the Pancake Tortoise, *Malacochersus tornieri* in Kenya. *Journal of East African Natural History*, **92**(1): 81–96.

(10) Jiří Moravec

Department of Zoology, National Museum, 115 79 Prague 1, Czech Republic

(e-mail: jiri_moravec@nm.cz)

I would like to support the arguments described by Jack Frazier in Case 3463 to conserve the name *Testudo gigantea* Schweigger, 1812. The Aldabra tortoise is a particularly well known species and its specific name *gigantea* has been widely used for more than 100 years. In this case all proposals to supplant this name by other names (e.g. *Testudo dussumieri* Gray, 1831; *T. elephantina* Duméril & Bibron, 1835) would lead to nomenclatural chaos negatively affecting conservation and education activities. Regarding the discovery of Schweigger's purported type specimen (Bour, 2006) the name *Testudo gigantea* Schweigger, 1812 should be retained as the conserved name. Acceptance of the neotype designation (Frazier 2006a) can easily solve this issue.

(11) Malcolm Penny

The Old Tavern, Union Road, Smallburgh, Norfolk NR12 9NH, U.K.

(e-mail: malcolm@mpenny.plus.com)

I should like to add my name to those who support Jack Frazier in his attempt to stabilise the name of *Testudo gigantea* for the Aldabra Giant Tortoise. I worked with Dr Frazier on the atoll in the 1960s, and I have the greatest respect for his opinions. Any move which makes the identification of such an important species more complicated by confusing nomenclature should be resisted in the interest of conservation.

(12) Peter Praschag

Behler Chelonian Center (The Turtle Conservancy), P.O. Box 1289, Ojai, California 93024, U.S.A. (e-mail: peter@praschag.at)

I have been following the ongoing debate about the correct naming of the Aldabra tortoise. In awareness of the ICZN Code and by carefully studying Case 3463, I strongly support the diligent application by Jack Frazier to stabilise the nomenclature by using the name *Geochelone (Aldabrachelys) gigantea* (Schweigger, 1812). The argument that Hubrecht (1881) and Boulenger (1889) had used the species name *gigantea* in association with Aldabra Atoll, and that *T. gigantea* Schweigger, 1812 has been consistently recognised as the oldest available name for the Aldabra tortoise for more than 50 years, is compelling. Furthermore there is no doubt that *Geochelone (Aldabrachelys) gigantea* is overall the most widely used name for the species. Bour's (1982) assumption that *T. gigantea* is a junior synonym of *Cylindraspis indica* (Schneider, 1783) and the rediscovery of the holotype have been causing instability in nomenclature, which is counterproductive for any conservation effort for a species facing various threats. I can only agree that the simplest and most maintainable solution is to recognise USNM 269962 as the neotype of *T. gigantea* and to suppress *T. dussumieri* Gray, 1831.

(13) Pavel Šíroký

Department of Biology and Wildlife Diseases, Faculty of Veterinary Hygiene and Ecology, University of Veterinary and Pharmaceutical Sciences Brno, Palackého 1–3, 612 42 Brno, Czech Republic (e-mail: sirokyp@vfu.cz)

I have to support the arguments published by Jack Frazier in Case 3463 to conserve the name *Testudo gigantea* Schweigger, 1812 for the Aldabra giant tortoise. *Testudo gigantea* (now *Geochelone gigantea* or *Aldabrachelys gigantea*) has been widely used as the scientific name for this species for over a century. This name was replaced first by *Testudo elephantina* Duméril & Bibron, 1835 and then by *Testudo dussumieri* Gray, 1831 during the last few decades. These changes introduce chaos and uncertainties into the diverse research and educational communities interested in chelonians, because not only taxonomists are dealing with tortoises. There are many fields that overlap with taxonomy in general biological sciences, e.g. ecology, parasitology, nature conservation, etc. Even herpetoculturists and tortoise fans are suffering from this situation. That is why I vote for the name *Testudo gigantea* Schweigger, 1812 to be retained as a nomen conservandum, and for acceptance of the neotype designation, which can solve the problem.

(14) Phillip Q. Spinks

Section of Evolution and Ecology, University of California, Davis, CA 95616, U.S.A. (e-mail: pqspinks@ucdavis.edu)

Jack Frazier has diligently and thoroughly recounted the quagmire that is the taxonomy of Aldabra tortoises. Having read Frazier's summary, I am in complete agreement with his proposal to designate a neotype for this species, and agree with the suppression of the name *dussumieri*. Such drastic measures are necessary to stabilise the taxonomy of this group.

(15) Christine Griffiths

School of Biological Sciences, University of Bristol, Woodland Road, Bristol BS8 1UG, U.K. (e-mail: Christine.Griffiths@bristol.ac.uk)

I support the petition to conserve the name of the Aldabran giant tortoise *Testudo gigantea*, particularly advocating the use of the specific name *gigantea*. The inconsistency in the nomenclature leads to confusion and frustration for readers familiar, and more so unfamiliar, with the volatility of the Latin name. Upon initially embarking on my studies with Aldabran giant tortoises, I repeatedly had to change the Latin name to satisfy different co-authors. While more than one name exists, dissatisfaction with a chosen nomenclature will persist. Consequently stabilising the name will be a relief to future work and aid in conformity with regards to referencing.

(16) Dennis Hansen

Department of Biology, 371 Serra Mall, Stanford University, Stanford, CA 94305–5020, U.S.A. (e-mail: dmhansen@stanford.edu)

I write to voice my strong support of the petition by Frazier (Case 3463) to conserve the usage of *Testudo gigantea* Schweigger, 1812 (and thus, by default, *Aldabrachelys*) for the Aldabra giant tortoise. As a conservation ecologist working with these tortoises on other islands in the Western Indian Ocean, it has been frustrating, to say the least, to have to sift through the nomenclatural chaos surrounding the Aldabra giant tortoise. Together with my collaborators, I consistently use *Aldabrachelys gigantea* (see, for example, additional references below) as the best available name: ‘*gigantea*’ because it has been used in the majority of peer-reviewed scientific papers dealing with the Aldabra giant tortoise in the last five decades, and ‘*Aldabrachelys*’ because the genus *Geochelone* is obviously rampantly polyphyletic, and will likely cease to exist in its present form within very few years indeed. I have refrained from using either *Dipsochelys* or *dussumieri* as I found these to have been promoted only recently, by very few authors, and often in non-peer-reviewed herpetological books, or journals with a local focus. To me, the most parsimonious solution to the current mess is to accept Frazier’s (2006) designation of a neotype to once and for all fix the name that fits the tortoise best. I cannot agree with Gerlach’s (BZN 66: 185–186) discourse that ‘*Aldabrachelys*’ is an inappropriate name: after all we think of this animal as the ‘Aldabra giant tortoise’, and not ‘Dussumier’s thirsty tortoise’.

Additional references

- Hansen, D.M., Kaiser, C.N. & Müller, C.B.** 2008. Seed dispersal and establishment of endangered plants on oceanic islands: The Janzen-Connell Model, and the use of ecological analogues. *PLoS ONE*, 3(5): e2111. doi:10.1371/journal.pone.0002111. <http://www.plosone.org/article/info:doi/10.1371/journal.pone.0002111> (accessed 17 July 2009).
- Kaiser-Bunbury, C.N., Traveset, A. & Hansen, D.M.** (in press). Conservation and restoration of plant-animal mutualisms on oceanic islands. *Perspectives in Plant Ecology Evolution and Systematics*.
- Griffiths, C.J., Jones, C.G. Hansen, D.M., Puttoo, M., Tatayah, R.V., Müller, C.B. & Harris, S.** (in press). The use of extant non-indigenous tortoises to replace extinct ecosystem engineers: a restoration tool. *Restoration Ecology*.

(17) Kirsten Bauerfeld

Tierpark Berlin-Friedrichsfelde GmbH, Am Tierpark 125, 10319 Berlin, Germany
(e-mail: k.bauerfeld@tierpark-berlin.de)

I was informed of Jack Frazier's application to stabilise the name of the Aldabran tortoise to *Geochelone gigantea*. Of course, I support this application. I am not a herpetologist, but as the director of both Berlin Zoos (Zoo and Tierpark) which are still keeping (Tierpark) or have kept (Zoo) Aldabra tortoises I am very interested in the question of the scientific name of the Aldabra tortoise. With 1463 species Berlin Zoo is the most species-rich zoo in the world, and together with the 950 species in the Tierpark we show over 3.9 million visitors a collection of biodiversity which is unique in the world. I am sure the name of the Aldabra tortoise should be *Geochelone gigantea* and so I wholeheartedly support Jack Frazier's application. We use this name in our publications (*Guidebook*, Annual Report, *Milu*) and in the labelling for our tortoise enclosures.

(18) Frank Glaw

Zoologische Staatssammlung München, Münchhausenstr. 21, 81247 München, Germany (e-mail: frank.glaw@zsm.mwn.de)

The permanent confusion about the correct scientific genus and species name of this prominent species is not acceptable in my opinion. Cases like this might give the impression that the current system of zoological nomenclature is not appropriate for the requirements of the 21st century and might finally lead to requests for a complete replacement of the Linnean nomenclature by a more efficient system that is entirely liberated from the burdens of history. The designation of a neotype of *Testudo gigantea* by Frazier was a reasonable act to resolve and finish the nomenclatural confusion concerning this species, at both the genus and species level. Therefore I strongly support the application of Frazier (Case 3463) for the sake of nomenclatural universality and stability.

(19) Jonathan F. Fong

University of California Berkeley, Museum of Vertebrate Zoology, Valley Life Sciences Building, Berkeley, CA 94720-3160 U.S.A. (e-mail: j_fong@berkeley.edu)

I am writing in support of Jack Frazier's petition to stabilise the name of the Aldabra tortoise by retaining the names *Aldabrachelys* and *Testudo gigantea*, and rendering *Testudo dussumieri* invalid. Frazier clearly and convincingly argues his case in the petition. In this case, *Aldabrachelys* and *Testudo gigantea* have been the norm in the scientific community when referring to the Aldabra tortoise. Changing the name based on a dubious holotype would cause terrible confusion, unnecessarily complicating future conservation and research.

(20) Uwe Fritz

Museum of Zoology, Senckenberg Dresden, A.B. Meyer Building, D-01109 Dresden, Germany (e-mail: uwe.fritz@senckenberg.de)

Otto Kraus

Universität Hamburg, Zoologisches Institut und Zoologisches Museum, Martin-Luther-King-Platz 3, D-20146 Hamburg, Germany

In the heated debate on the question of whether the name *Testudo gigantea* Schweigger, 1812 should be conserved for the Aldabra tortoise there are two lines of argumentation visible: one fraction represented by Frazier (BZN 66: 34–50, 2009) and his many supporters suggests stabilising the well established name *Testudo gigantea* Schweigger, 1812 for the Aldabra tortoise, a widely known, charismatic species of common interest. Frazier presented convincing evidence for the long and frequent usage of the species name *gigantea* and we fully support his application. The other fraction, represented by Bour & Pritchard (BZN 66: 169–174, 2009) and supported by only a few others (Cheke, BZN 66: 174–176, 2009; Gerlach, BZN 66: 184–186, 2009) earlier involved in proposing or supporting competing names (e.g. Gerlach, 2004a; Cheke & Hume, 2008), challenges the stability of nomenclature and argues on the basis of historical reasons for proper allocation of the identity of a type specimen that was lost for more than a century in the Paris museum but rediscovered just when it was supportive for their point of view.

Considering the fundamental aim of the Code (1999, p. XIX), ‘which is to provide the maximum universality and continuity in the scientific names of animals...’, then there can be only one decision: to support the application by Frazier to conserve the usage of *Testudo gigantea* Schweigger, 1812.

As a logical consequence, when *Testudo gigantea* Schweigger, 1812 is accepted as valid for the Aldabra tortoise, the name *Aldabrachelys* Loveridge & Williams, 1957, based on the type species *Testudo gigantea* Schweigger, 1812, becomes its generic name if the species is placed in a distinct genus. Accordingly, *Dipsochelys* Bour, 1982 is a junior subjective synonym of *Aldabrachelys* Loveridge & Williams, 1957.

Additional references

Cheke, A.S. & Hume J.P. 2008. *Lost land of the Dodo: An ecological history of Mauritius, Réunion and Rodrigues*. 464 pp. A & C Black, London & Yale University Press. New Haven, Connecticut.

(21) Adrian Hailey

Department of Life Sciences, The University of the West Indies, St. Augustine, Trinidad and Tobago, West Indies (e-mail: adrian.hailey@sta.uwi.edu)

I wish to express my support for the conservation of the specific name *gigantea* for the tortoise *Geochelone gigantea*. I have used that name in several publications on ecology and physiology of tortoises, in addition to the two quoted in Case 3463, and believe that the taxonomic case for the new name *Dipsochelys dussumieri* is not sound.

(22) Miguel Vences

Technical University of Braunschweig, Zoological Institute, Spielmannstr. 8, 38106 Braunschweig, Germany (e-mail: m.vences@tu-bs.de)

Having been involved in intensive taxonomic revisionary work on the amphibians and reptiles of Madagascar over the past 15 years, I have also been following peripherally but regularly the taxonomic discussions surrounding the giant tortoises of the Indian Ocean archipelagos. More than once I have felt distressed about the discordances on both generic and specific names to be used, due to both taxonomic and nomenclatural disagreements. I feel that stabilising the situation is an urgent matter, and I therefore applaud and fully support the proposal of J. Frazier to stabilise the name of the Aldabra tortoise, even if other colleagues probably are right that another name may have nomenclatural priority. Considering the disagreements in the past, it is unlikely that the scientific community will reach a consensus on which name to use without an unambiguous decision of the Commission – and such a consensus is badly needed to make discussions on the evolution, ecology and conservation of these fascinating creatures available to a wide audience.

(23) Ross M. Wanless

Percy FitzPatrick Institute, Zoology Building, University of Cape Town, Rondebosch 7701, South Africa (e-mail: rosswanless@gmail.com)

I would like to express my unreserved support for the proposal by J. Frazier to conserve the name of the Aldabra Giant Tortoise as *Testudo gigantea* Schweigger, 1812 by the designation of a neotype. There is clearly an urgent need for resolution of this extremely confusing situation. I found myself, as a practising biologist with interest and experience in taxonomic matters, too easily swayed by plausible-sounding explanations and publications. However, as a non-specialist I was ill-equipped to judge the merits of these apparently spurious arguments. Frazier's cogent and comprehensive treatment has rectified this situation. I have read in great detail the case presented by Frazier, and I am convinced of its merits.

I have previously published about this species using the name *Dipsochelys dussumieri* (Wanless, 2002; Wanless & Hockey, 2008). This was done through innocent misperception, based on personal communications with R. Bour, that the definitive solution to the taxonomic confusion had been accepted. I request that these publications be struck from the list of those in support of the use of either *Dipsochelys* or *dussumieri*. All future publications of mine will use the specific name *gigantea* until the Commission has reached a final decision. I urge acceptance of Frazier's neotype and proposed retention of *gigantea*.

Additional references

- Wanless, R.M. 2002. *The reintroduction of the Aldabra Rail Dryolimnas cuvieri aldabranus to Picard Island, Aldabra Atoll*. 156 pp. (MSc, thesis). Percy FitzPatrick Institute of African Ornithology, Zoology Department, University of Cape Town.
- Wanless, R.M. & Hockey, P.A.R. 2008. Natural history and behavior of the Aldabra Rail (*Dryolimnas [cuvieri] aldabranus*). *The Wilson Journal of Ornithology*, **120**: 50–61.

(24) Ernest H. Williams Jr.

Department of Marine Sciences, University of Puerto Rico, P.O. Box 9000, Mayagüez, Puerto Rico 00680–9000 (e-mail: ernest.williams1@upr.edu)

Ángel M. Nieves-Rivera

Juan A. Rivero Zoological Garden, Bo Miradero Carr 108 Interior, Mayagüez, Puerto Rico 00680 (e-mail: anievesster@gmail.com)

Lucy Bunkley-Williams

Department of Biology, University of Puerto Rico, P.O. Box 9000, Mayagüez, Puerto Rico 00680–9000 (e-mail: lucy.williams1@upr.edu)

We have examined Aldabra giant tortoises in the wild (EHW and LBW) as well as in captivity (AMNR) and discussed crab-tortoise interactions (Nieves-Rivera & Williams, 2003) as may even have once occurred on Mona Island (Williams, 1952). In Nieves-Rivera & Williams (2003) we referred to the Aldabra Giant Tortoise as *Dipsochelys dussumieri* (Gray, 1831) instead of the more accustomed name *Geochelone* (*Aldabrachelys*) *gigantea* (Schweigger, 1812). We did not intend our use of this name as an endorsement of the validity of *Dipsochelys dussumieri*. After reading Case 3463 we now accept *Geochelone gigantea* as the most appropriate name for this species. We support the petition to conserve the specific name *Testudo gigantea* Schweigger, 1812 for the Aldabra Giant Tortoise and agree with Frazier that *T. gigantea* is the established name, as explained in Case 3463. The neotype for *T. gigantea* (USNM 269962) that was designated in 2006 should be maintained.

Additional references

Nieves-Rivera, Á.M. & Williams, E.H., Jr. 2003. Annual migrations and spawning of *Coenobita clypeatus* (Herbst) on Mona Island (Puerto Rico) and notes on island crustaceans. *Crustaceana*, **76**: 547–558.

(25) Ute Grimm

CITES Animals Committee, German Scientific Authority (Fauna), German Federal Agency for Nature Conservation, Konstantinstr. 110, 53179 Bonn, Germany (e-mail: Ute.Grimm@BfN.de)

I support the petition of Jack Frazier to conserve the names *Aldabrachelys* and *Testudo gigantea* for the Giant Aldabra Tortoise and suppress *T. dussumieri* Gray, 1831. As outlined in the comment by Frazier the name *gigantea* has been used for more than 100 years in scientific publications. A change could lead to quite a confusion. That name has also been used in conservation publications and international law. The species has been listed under the name *gigantea* in Appendix II of CITES (Convention on International Trade in Endangered Species of wild animals and plants) since 1975 as well as in many national laws of CITES Parties. I share the opinion of Anders Rhodin that not suppressing *T. dussumieri* could increase the threat to this species by creating potential loophole exceptions in national and international laws protecting this species.

(26) John B. Iverson

Department of Biology, Earlham College, Richmond, IN 47374, U.S.A.
(e-mail: johni@earlham.edu)

I am writing in response to Case 3463 (2009, BZN 66: 34–50) currently before the Commission. This proposal by Frazier recommends retaining his designated neotype for *Testudo gigantea* Schweigger, 1812. However, this has been rendered completely unnecessary by the rediscovery of the holotype of *T. gigantea* in the Paris Museum (MNHN 95541) by Bour (2006) (see also Bour & Pritchard, BZN 66: 169–174). I was a reviewer of Bour & Pritchard's [2009] paper, and I am convinced that the authors have indeed rediscovered the long-misplaced holotype, making the name *T. gigantea* a junior synonym of *T. denticulata*, and thus unavailable for any tortoise native to the Indian Ocean (including the Aldabra tortoise). Hence, I am surprised and perplexed by the onslaught of opinions favouring Frazier's petition, when most of the opinions have no reference to or basis in the International Code of Zoological Nomenclature, which is the foundation of proper zoological names.

Most of the published comments were written by non-taxonomists, many of whom have apparently never read the Code. Rather, their comments seem to be based on personal preference for a name which they have grown accustomed to using. However, personal preference for or personal comfort with zoological names has (or should have) little value in nomenclatural decisions when they are at odds with the Code. Logical nomenclatural decisions, guided explicitly by the Code, are essential if we are ever to have taxonomic stability. Practising law without reading or understanding legal statutes is unacceptable; why should practising nomenclature with no basis in the Law (Code) be any different? It further concerns me that many authors and editors are not being held to higher standards in recognising currently accepted names. As but one example, note the use of *Geochelone elephantopus* for the Galapagos tortoise in Poulakakis et al. (2008), when the accepted name is *Chelonoidis nigra* (see Pritchard, 1986, 1996; Le et al., 2006; Fritz & Havas, 2007; Turtle Taxonomy Working Group, 2007; Rhodin et al. 2008 among many others).

In my opinion, Bour (2006) and Pritchard (1986) (see also BZN 66: 169–174; Bour & Pritchard [2009]) have both read and correctly interpreted the Code regarding 1) the inappropriateness of designating a neotype for *Testudo gigantea*, 2) the valid species name for the Aldabra tortoise (*dussumieri*), and 3), based on currently understood phylogenetic relationships (Le et al., 2006; among others), the valid genus name (*Dipsochelys*). I urge the ICZN not to be swayed by 'public opinion', but instead to rule against the conservation of *Testudo gigantea* for the Aldabra tortoise, based on a critical reading of the Code.

Additional references

- Bour, R. & Pritchard, P.C.H. [2009] The identity of *Testudo gigantea*, 1812. *Zootaxa*, [in press].
- Le, M., Raxworthy, C.J., McCord, W.P. & Mertz, L. 2006. A molecular phylogeny of tortoises (Testudines: Testudinidae) based on mitochondrial and nuclear genes. *Molecular Phylogenetics and Evolution*, 40: 517–531.
- Poulakakis, N., Glaberman, S., Russello, M., Beheregaray, L.B., Ciofi, C., & Caccone, A. 2008. Historical DNA analysis reveals living descendents of an extinct species of Galápagos tortoise. *Proceedings of the National Academy of Sciences*, 105: 15464–69.

Pritchard, P.C.H. 1996. The Galápagos tortoises: Nomenclatural and Survival Status. *Chelonian Research Monographs*, 1: 1–85.

(27) Jeanne A. Mortimer

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Systematic nomenclature was devised, not for the amusement of taxonomists, but to serve a real need in society to classify the elements of our biosphere in a way that eliminates confusion caused by common names. Sadly, the situation that currently exists for the Aldabra Tortoise is an example of taxonomy run amuck. The Aldabra Tortoise, a form of charismatic megafauna and the national animal for the Republic of Seychelles, is of great interest to virtually everyone in our country, be they conservationists, politicians, school children, visiting tourists, or the general public. Yet, for the past quarter century the vast majority of us who are not professional taxonomists have felt uncertain about which binomial to apply when publishing papers, preparing exhibits, drafting legislation, producing permits, giving talks, or writing popular accounts. A small number of people involved in taxonomic pursuits, on the other hand, seem to have revelled in each new opportunity to promote their current favourite binomial.

It is for these reasons I fully support the proposal of Jack Frazier (Case 3463) to stabilise the species name *gigantea* for the Aldabra Tortoise. Frazier's petition is based on the premise that stability of scientific names is essential and in many cases more important than trying to interpret the true taxonomic intent of the author of a name, and his designation of a neotype (Frazier, 2006a) for *T. gigantea* and submission of Case 3463 are based on a clear following of the Principles and Articles of the Code, in the latter case Article 75.8. The latter allows previous type material to be set aside and designation of another name-bearing type to act as the reference for a species name when the previous type material is not helpful in understanding what species is being dealt with. Moreover, Frazier's intention to conserve *gigantea* is entirely appropriate given that *gigantea* is indeed the most frequently used species name for the Aldabra tortoise, especially during the latter half of the 20th century with the publication of a massive body of field research conducted at Aldabra atoll.

Bour's attempt to override Frazier's neotype designation with his claimed rediscovery of Schweigger's long lost holotype, a specimen that is clearly not an Aldabra tortoise (Bour, 2006), is problematic in that it fuels continued debate and nomenclatural chaos. I hope that the Commission will accept the recommendations in Case 3463 to stabilise the name of the Aldabra Tortoise as *gigantea* by maintaining the neotype (USNM 269962). In doing so, they also will grant those of us working on pressing issues of conservation on the ground in Seychelles a much needed respite from confusion and a long overdue sense of stability.

(28) James Buskirk

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The arguments presented in favour of the suppression of the binomen *Dipsochelys dussumieri* Gray, 1831 are compelling in their presentation of a thorough and objective review of this admittedly complex nomenclatural debacle. Frazier made an eloquent case on behalf of stabilisation of the nomenclature of the Aldabra tortoise in 2006, with his designation of a neotype for *Testudo gigantea* Schweigger, 1812. Case 3463 is largely a cogent re-statement of the arguments presented therein, with the addition of scepticism concerning the rediscovery in the MNHN Paris of the specimen claimed by Bour (2006) to be Schweigger's *T. gigantea* – actually a yellow-foot tortoise *Chelonoidis (Geochelone) denticulata*. This large specimen, previously overlooked by Bour and by all workers since Duméril and Bibron in 1835, may have been the tortoise brought to Paris from Lisbon in 1808 as claimed. However, Fretey (1977) was familiar with this specimen (MNHNP 9554), and included it without comment nor mensural data in his section devoted to *G. denticulata* (Fretey, 1977, p. 54). Thus, more uncertainty and contradiction plague the quest for clarity with regard to choosing the best binomen for the Aldabra tortoise. For the sake of stabilisation, the suppression of the binomen *Dipsochelys dussumieri* remains the wisest choice, and I suggest the Commission consider adopting the recommendations expressed in Frazier's point 29.

In 1986, both Pritchard and Crumly published papers on the topic of proper nomenclature for the Aldabra tortoise, espousing different points of view based largely on differing interpretations of Schweigger's text. Until very recently I had had no familiarity with Crumly's paper, and find his proposal in favour of adopting *Testudo (Aldabrachelys) gigantea* more cogent and succinct than Pritchard's eloquent but rambling endorsement of the species epithet *elephantina* or, as a second choice, *dussumieri*, based on Bour's thrice-published arguments.

There is more value in the stabilisation of an accepted nomenclature than in embracing the speculation and shards of certainty offered to justify the neologism *Dipsochelys dussumieri*. I urge you instead to approve the binomen enjoying widest currency, *Aldabrachelys gigantea*, for the Aldabra tortoise.

Additional references

Fretey, J. 1977. *Les Chéloniens de Guyane française 1. Étude préliminaire. Mémoire présenté à l'université Paris VI pour l'obtention du diplôme d'études supérieures de sciences*. 201 pp. Paris.

(29) J. Morgan

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I am writing as the Minister responsible for Environment on behalf of the Government of Seychelles. It has come to our attention that a petition has recently been submitted to the International Commission on Zoological Nomenclature (ICZN): 'Case 3463 – *Testudo gigantea* Schweigger, 1812 (currently *Geochelone (Aldabrachelys) gigantea*; Reptilia, Testudines): proposed conservation of usage of the specific name by maintenance of a designated neotype, and suppression of *Testudo dussumieri* Gray, 1831 (currently *Dipsochelys dussumieri*),' published in this year's *Bulletin of Zoological Nomenclature* (BZN 66(1): 34–50) and submitted by Dr J. Frazier of the Smithsonian Institution.

This issue is of great relevance to the Republic of Seychelles for it is our sovereign country – and only our country – in which the animal in question lives in the wild. The place where this endangered tortoise lives, Aldabra Atoll, was inscribed into the World Heritage List in 1982, an international initiative coordinated by the *United Nations Educational, Scientific and Cultural Organization* (UNESCO) in implementation of the *United Nations' Convention Concerning the Protection of the World Cultural and Natural Heritage*. Of all the forms of life that live on Aldabra Atoll and indeed in the entire country of Seychelles, it is the Aldabra giant tortoise that is most famous and the most iconic.

The Government of Seychelles does not wish to dictate to scientists what they have to do. But it is our responsibility and duty to make every effort to protect our natural resources, particularly unique forms of life that only occur in our country, as well as the areas in which they live and on which they depend. The Government of Seychelles's dedication and commitment to the environment is globally recognised and we are enormously proud to be the stewards of this unique animal and the unique Aldabra Atoll. However, protection cannot be effective if scientists cannot make up their minds about something such as the correct name to be used for the Aldabra tortoise. Legislation, customs activities, international relations, educational activities, budgets and policy decisions all become unnecessarily complicated, confused and unproductive, and difficult to administer when there are many different scientific names in circulation.

We note that Case 3463 provides an extremely detailed analysis of the scientific names involved with the long history of the Aldabra tortoise. It is not our place to dictate on the rules of zoological nomenclature but it is our place to guarantee that our wildlife is adequately understood and protected. It is clearly explained in Case 3463 that for a variety of reasons the species' name that has been established for the Aldabra tortoise for more than a century is *gigantea*. This is the name that appears in our legislation, in the legislation of other countries, in international treaties and a host of other official documents. For this reason, we respectfully request that you accept the petition made in Case 3463, aimed at stabilising the name of the Aldabra tortoise, using the established scientific name for the species.

Furthermore, to avoid needless confusion in the future, it is also necessary to invalidate the name that has come into increasing use over the last few years, a name that had been forgotten for more than 150 years. To do otherwise is to risk continuation of uncertainty.

(30) Fabian Schmidt

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I agree with Dodd (BZN 66: 179) that Case 3463 is really an excellent article. I am a scientifically educated biologist but work now in 'applied biology' as curator for reptiles at one of Europe's largest zoos (26 hectares, 870 animal species, 1.7 million visitors each year). I think naming of animals has so many consequences for further biological research, for education and especially for conservation and the national and international laws enabling conservation, that it is in the interest of all concerned professionals to follow and influence such discussions. In this respect I strongly agree with dozens of colleagues who have already commented on this point. I am often asked about the correct name for the Aldabra tortoise and I can't really tell them, because it is such a complicated story with so many names for just one species. However I always point out that in the official documents of international organisations such as CITES or IUCN or ISIS the species is still referred to as *Geochelone gigantea*. I am also Vice Chair of the Reptile Taxon Advisory Group (RTAG) within the European Association of Zoos and Aquaria (EAZA). This group gives advice to all European Zoos on reptilian questions. In our regional collection plan for EAZA we have listed the species as *Geochelone gigantea* and we do not intend to change that, because we have the same thoughts expressed in Case 3463 regarding the need for nomenclatural stability. Furthermore as public zoos we have a great responsibility in educating our visitors. It is very important to understand the unique impact that zoos in the World Association of Zoos and Aquariums have on public education; it is estimated that more than 600 million visitors come to zoos each year. I know from personal visits to hundreds of zoos that the specific name *gigantea* is used by the huge majority when referring to the Aldabra tortoise. From this you can imagine how many people around the world have come to know the Aldabra tortoise by the species name *gigantea*. This fact is actually recognised by the few people who are proposing to change the name of this animal, and Case 3463 clearly gives several examples where people proposing another name (BZN 66: 40) have found it necessary to clarify that the same animal is more frequently known by the name *gigantea*. Additionally, our zoos provide training and assistance to customs officers in many countries for enforcement of conservation and trade laws. For these purposes we need a name that remains stable, and over the years we have always referred to the Aldabra tortoise by the species name *gigantea*. Changing this name produces confusion and chaos, just where we need stability and universality to be able to strictly implement laws and regulations.

Even if it should turn out that the holotype of *Testudo gigantea* Schweigger (1812) is not an Aldabra tortoise, there is still the central importance that the ICZN Code gives to stability and universality. In my opinion, the Preamble in the Principles of the Code declares the nomenclatural stability as an overriding concern of the ICZN.

So the most important question here is what is stability? Bour & Pritchard (BZN 66: 169) resist letting usage, consensus and majority decide about a scientific name. But aren't usage, consensus and majority the features that characterise stability? The debate about Case 3463 is not about whether some name has priority over *gigantea* because this name is clearly older than the alternate names that have been proposed

for the Aldabra tortoise, but Cheke's comment indicates that he appreciates the importance of nomenclatural stability. He admits in his comment (BZN 66: 174), that 'This [using a name that does not conform to the rule of priority] should surely only be done if there is an absolutely cast-iron case to preserve a thoroughly established name, the loss of which would cause substantial confusion and upheaval amongst users'. Does this situation with the species name of the Aldabra tortoise not already now cause substantial confusion and upheaval amongst users? This is even accepted by one of the opponents: Pritchard (1986) wrote 'Of course, invalidation of the familiar epithet *gigantea* represents a rather profound upheaval'. Personally I can't recognise a significant difference in the meanings of the adjectives substantial and profound. Gerlach writes (BZN 66: 184) 'that a very strong case [for using a name that does not conform the rule of priority] would be needed for such a change to be acceptable'. Unfortunately he doesn't provide any examples, whereas in another comment Bour & Pritchard (BZN 66: 170) refer to *Drosophila melanogaster*, *Tyrannosaurus rex* and *Homo erectus*. I do not understand why these three species are regarded in a different view from *Geochelone gigantea*. Is *Drosophila melanogaster* more charismatic than *Geochelone gigantea*? Do laymen know more about *Drosophila melanogaster* than about *Geochelone gigantea*? Is *Homo erectus* really more often in the mass media than *Geochelone gigantea*, of which reports about 'birthday parties' for oldest inhabitants of zoological collections are regularly in the newspapers around the globe? At least I can tell from my experience of daily work in a zoo that a giant tortoise is one of the most popular animal species visitors are looking for.

I was very surprised to read arguments in the comments on Case 3463 for and against different genus names. As far as I know, the genus name is not a topic for this ICZN decision, other than making sure that *Aldabrachelys*, which was specifically created by Loveridge & Williams (1957) for the Aldabra tortoise and its close relatives, is available for this taxon. I also have difficulty understanding why authors who resist letting 'usage, consensus and majority' influence the name *gigantea*, in the same comment are in favour of *Dipsochelys* because '*Aldabrachelys* was rarely used until recently' (Bour & Pritchard, BZN 66: 173), a declaration which contradicts an earlier statement by Pritchard (1986, p. 532) that *Aldabrachelys* has been 'in regular use'. I don't know why 'usage, consensus and majority' should be considered for a genus but not for a species name.

Stability in the name of this unique and charismatic species is wished for by all involved parties, and the only way to reach this aim is by giving it the name it deserves, *Geochelone gigantea*.

Stability of the name of this unique and charismatic species is wished for by all parties involved. When this issue is finally resolved, I am anticipating that many unresolved biological questions and necessary conservation issues will be addressed more thoroughly. Personally I don't think stability will be established by the name *dussumieri*, as many professionals are not very familiar with decisions in nomenclatural and taxonomic debates. Therefore I think the only way to reach stability is by giving the Aldabra tortoise the name it deserves, *Geochelone gigantea*.

(31) Jeff Miller

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I support the application of Frazier (2009, Case 3463) to conserve the specific name *Testudo gigantea* Schweigger, 1812.

Frazier (2009) provided a detailed argument concerning the nomenclatural history, confusion, and instability, including that caused as a result of a recent paper that I published (Miller & Dinkelacker, 2007) concerning turtle reproductive structures and strategies. The focus of the chapter was not taxonomic but the taxonomy should have been consistent; it was not and the error is mine. The fact that other people reviewed the manuscript and missed the inconsistency/error in the nomenclature (or at least did not comment on the taxonomy to me) suggests that I am not the only one who would benefit from a stabilised taxonomy.

Additional references

Miller, J.D. & Dinkelacker, S.A. 2007. Reproductive structures and strategies of turtles. Pp. 225–278 in Wyneken, J., Godfrey, M.H. & Bels, V. (Eds.), *Biology of turtles: From structures to strategies of life*. CRC Press, Boca Raton, Florida.

OPINION 2230 (Case 3400)***Hybosorus illigeri* Reiche, 1853 (Insecta, Coleoptera): precedence not given over *Hybosorus roei* Westwood, 1845**

Abstract. The Commission has ruled that priority is maintained for the species name *Hybosorus roei* Westwood, 1845. A proposal to conserve the junior synonym *Hybosorus illigeri* Reiche, 1853 by giving it precedence over *Hybosorus roei* Westwood was not approved.

Keywords. Nomenclature; taxonomy; HYBOSORIDAE; *Hybosorus illigeri*; *Hybosorus roei*; *Hybosorus pinguis*; *Hybosorus carolinus*; scarabaeoids.

Ruling

- (1) It is hereby ruled that the name *roei* Westwood, 1845, as published in the binomen *Hybosorus roei*, retains priority over the name *illigeri* Reiche, 1853, as published in the binomen *Hybosorus illigeri*, whenever the two are considered synonyms.
- (2) No names are placed on the Official Lists or Indexes in this ruling.

History of Case 3400

An application to conserve the specific name *Hybosorus illigeri* Reiche, 1853 for a well known, widely distributed scarabaeoid beetle (HYBOSORIDAE) by giving it precedence over the little-used senior subjective synonym *Hybosorus roei* Westwood, 1845 and unused senior subjective synonyms, *Hybosorus pinguis* Westwood, 1845 and *Hybosorus carolinus* LeConte, 1847, was received from P.G. Allsopp (*BSES Limited, Indooroopilly, Australia*) and T. Branco (*Porto, Portugal*) on 3 December 2006. After correspondence the case was published in BZN 64: 90–95 (June 2007). The title, abstract and keywords of the case were published on the Commission's website. No comments were received on this case.

Decision of the Commission

The case was sent to the members of the Commission on 1 March 2008. The case received a majority of votes cast (11 FOR, 9 AGAINST), but failed to reach the two-thirds majority required for approval.

In accordance with Bylaw 35, the proposals published in BZN 64: 92 were resubmitted to the Commission on 1 December 2008. At the close of the voting period on 1 March 2009 the votes were as follows:

Affirmative votes – 8: Brothers, Fautin, Grygier, Halliday, Krell, Minelli, Papp and Zhang.

Negative votes – 11: Alonso-Zarazaga, Bogutskaya, Kottelat, Kullander, Lamas, Lim, Ng, Pape, Rosenberg, Štys and van Tol.

Bouchet abstained. Patterson and Pyle were on leave of absence.

In the first round of voting Kullander, voting AGAINST, said that Paragraph 8 of the application showed that the taxonomy of the name had not been worked out, so

the case was not ripe for Commission consideration. Štys, also voting AGAINST, said that a modern taxonomic revision should precede the requested decision—under the circumstances it seemed better to follow the Principle of Priority. He said that the authors of the application should have compared the name-bearing types of the two names involved and included information on their availability and location. Voting AGAINST, Kottelat pointed out that formally, as proposed in paragraph 7 of the application, *H. pinguis* and *H. carolinus* were not nomina oblita. Article 23.9.2 explicitly requires that the authors ‘state explicitly...that the action is taken in accordance with this Article’ and as Article 23.9.2 was not cited, the act was invalid. This meant that reversal of precedence between *H. illigeri* and *H. roei* would result, in fact, in giving precedence to *H. pinguis* and *H. carolinus*.

In the second round of voting Alonso-Zarazaga, voting AGAINST, said that he felt this case lacked a sound taxonomic basis. Furthermore, he suggested the case as it was presented provided no basis for suggesting the status of *Hybosorus pinguis* and *H. carolinus* as nomina oblita. He was concerned that no information was given about their type material and identity. Bouchet, who ABSTAINED, said he had earlier voted FOR the proposals, but was now disturbed to learn that the case was not adequately supported by modern taxonomic work; thus he abstained. Ng, voting AGAINST said he felt the circumstances were such that he did not see the need for plenary action. He was of the opinion that the evolving taxonomy would likely complicate things further if the Commission made a decision on this now.

Brothers, voting FOR, said that the declaration of *H. pinguis* Westwood, 1845 and *H. carolinus* LeConte, 1847 as nomina oblita in paragraph 7 of the application should be regarded as valid by implication of the context, notwithstanding the lack of an explicit reference to Article 23.9.2. Halliday, voting FOR, said he did not feel that the objections raised by other Commissioners were fatal to the proposal.

No names are placed on Official Lists or Indexes and the issue is left open for subsequent workers to follow the precepts of the Code or to make new proposals to the Commission.

OPINION 2231 (Case 3415)***Pedionomus* Gould, December 1840 (Aves, PEDIONOMIDAE) and *Leipoa* Gould, December 1840 (Aves, MEGAPODIIDAE): usage conserved**

Abstract. The Commission has suppressed the names *Pedionomus* and *Pedionomus ocellatus* Gould, [31 October] 1840, thereby conserving the generic names *Pedionomus* Gould, [1 December] 1840 for the Australian plains-wanderer (PEDIONOMIDAE) and *Leipoa* Gould, [1 December] 1840 and the specific name *Leipoa ocellata* Gould, [1 December] 1840 for the Australian malleefowl (MEGAPODIIDAE). The names *Pedionomus* Gould, [1 December] 1840, *Leipoa* Gould, [1 December] 1840 and *Leipoa ocellata* Gould, [1 December] 1840 have been in prevailing usage as the valid names for these taxa for over 150 years.

Keywords. Nomenclature; taxonomy; Aves; MEGAPODIIDAE; PEDIONOMIDAE; *Leipoa*; *Pedionomus*; *Leipoa ocellata*; *Pedionomus torquatus*; malleefowl; plains-wanderer; Australia.

Ruling

- (1) Under the plenary power it is hereby ruled that the following names are suppressed for the Purposes of both the Principle of Priority and the Principle of Homonymy:
 - (a) *Pedionomus* Gould, [31 October] 1840;
 - (b) *ocellatus* Gould, [31 October] 1840, as published in the binomen *Pedionomus ocellatus*.
- (2) The following names are hereby placed on the Official List of Generic Names in Zoology:
 - (a) *Pedionomus* Gould, [1 December] 1840 (gender: masculine), type species by monotypy *Pedionomus torquatus* Gould, [1 December] 1840;
 - (b) *Leipoa* Gould, [1 December] 1840 (gender: feminine), type species by monotypy *Leipoa ocellata* Gould, [1 December] 1840.
- (3) The following names are hereby placed on the Official List of Specific Names in Zoology:
 - (a) *ocellata* Gould, [1 December] 1840, as published in the binomen *Leipoa ocellata* (type species of *Leipoa* Gould, [1 December] 1840);
 - (b) *torquatus* Gould, [1 December] 1840, as published in the binomen *Pedionomus torquatus* (type species of *Pedionomus* Gould, [1 December] 1840).
- (4) The name *Pedionomus* Gould, [31 October] 1840, as suppressed in (1)(a) above is hereby placed on the Official Index of Rejected and Invalid Generic Names in Zoology.
- (5) The name *ocellatus* Gould, [31 October] 1840, as published in the binomen *Pedionomus ocellatus* and as suppressed in (1)(b) above, is hereby placed on the Official Index of Rejected and Invalid Specific Names in Zoology.

History of Case 3415

An application to conserve the generic names *Pedionomus* Gould, [1 December] 1840 for the Australian plains-wanderer (PEDIONOMIDAE) and *Leipoa* Gould, [1 December] 1840 and the specific name *Leipoa ocellata* Gould, [1 December] 1840 for the Australian malleefowl (MEGAPODIIDAE) by suppression of *Pedionomus ocellatus* Gould, [31 October] 1840, was received from R. Schodde (*Australian Biological Resources Study, Canberra, Australia*) and W.J. Bock (*Columbia University, New York, NY, U.S.A.*) on 23 October 2007. After correspondence the case was published in BZN 65: 31–34 (March 2008). The title, abstract and keywords of the case were published on the Commission's website. No comments were received on this case.

Decision of the Commission

On 1 March 2009 the members of the Commission were invited to vote on the proposals published in BZN 65: 33. At the close of the voting period on 1 June 2009 the votes were as follows:

Affirmative votes – 19: Bogutskaya, Bouchet, Brothers, Fautin, Grygier, Halliday, Kottelat, Krell, Lamas, Lim, Minelli, Ng, Pape, Papp, Patterson, Rosenberg, Štys, van Tol and Zhang.

Negative votes – 2: Alonso-Zarazaga and Kullander.

Pyle was on leave of absence.

Ng, voting FOR, thought that this was a very straightforward case and he fully agreed with the application. He said that changing the name because the 50-year rule failed in one instance is not a good justification—it would cause a great deal of unnecessary confusion. Alonso-Zarazaga, voting AGAINST, said he considered the application to start from a faulty point, that *The Athenaeum* was a published work in the sense of the Code, when in fact it failed to comply with the requirement of Article 8.1.1 that it must be issued for the purpose of providing a public and permanent scientific record. *The Athenaeum* was a newspaper and did not have this as its main purpose. *The Oxford Universal Dictionary Illustrated* defines newspaper as ‘a printed, now usually daily or weekly, publication containing the news, advertisements, literary matter, and other items of public interest’. Nothing indicates that newspapers are published for scientific record of any kind, which is common sense. If the Commission fails to recognise this, anything printed becomes ‘scientific record’ and there are thousands of newspapers in all world languages. Many of these may carry names and descriptions in advance of their publication in scientific (‘academic’) journals or books, especially in the case of ‘flagship’ or charismatic animals, like dinosaurs.

Original references

The following are the original references to the names placed on Official Lists and Indexes by the ruling given in the present Opinion:

Pedionomus Gould, [31 October] 1840, *The Athenaeum*, 679: 876.

ocellatus, *Pedionomus*, Gould, [31 October] 1840, *The Athenaeum*, 679: 876.

Pedionomus Gould, [1 December] 1840, *The birds of Australia*, Part 1. 17 pls. The author, London, text to pl. 17.

Leipoa Gould, [1 December] 1840, *The birds of Australia*, Part 1. text to pl. 6.

ocellata, *Leipoa* Gould, [1 December] 1840, *The birds of Australia*, Part 1. text to pl. 6.

torquatus, *Pedionomus*, Gould, [1 December] 1840, *The birds of Australia*, Part 1. text to pl. 17.

OPINION 2232 (Case 3384)***Cornwallius tabatai* Tokunaga, 1939 (currently *Paleoparadoxia tabatai*; Mammalia, Desmostylia): proposed designation of a neotype not accepted**

Abstract. The Commission has ruled that the usage of the specific name of *Paleoparadoxia tabatai* (Tokunaga, 1939), a species of Middle Miocene desmostylian from Japan and California, will not be conserved by designation of a neotype.

Keywords. Nomenclature; taxonomy; Desmostylia; PALEOPARADOXIIDAE; *Paleoparadoxia*; *Paleoparadoxia tabatai*; Middle Miocene; Japan.

Ruling

- (1) It is hereby ruled that the previous type fixation for the nominal species *tabatai* Tokunaga, 1939 in the genus *Cornwallius* Hay, 1923 is maintained.
- (2) No names are placed on the Official Lists or Indexes in this ruling.

History of Case 3384

An application to conserve the specific name of *Paleoparadoxia tabatai* (Tokunaga, 1939) for a species of Middle Miocene desmostylian from Japan and California was received from Y. Hasegawa (*Gunma Museum of Natural History, Gunma, Japan*) and N. Kohno (*Department of Geology and Paleontology, National Museum of Nature and Science, Tokyo, Japan*) on 11 May 2006. After correspondence the case was published in BZN 64: 113–117 (June 2007). The title, abstract and keywords of the case were published on the Commission's website. No comments were received on this case.

Decision of the Commission

The case was sent to the members of the Commission on 1 March 2008. The case received a majority of votes cast (9 FOR, 7 AGAINST), but failed to reach the two-thirds majority required for approval.

In accordance with Bylaw 35, the proposals published in BZN 64: 116 were resubmitted to the Commission on 1 December 2008. At the close of the voting period on 1 March 2009 the votes were as follows:

Affirmative votes – 6: Alonso-Zarazaga, Bogutskaya, Bouchet, Halliday, Papp and Zhang.

Negative votes – 13: Brothers, Fautin, Grygier, Kottelat, Kullander, Lamas, Lim, Minelli, Ng, Pape, Rosenberg, Štys and van Tol.

Krell abstained. Patterson and Pyle were on leave of absence.

Bouchet, voting FOR, felt the case had formal inconsistencies, but did not want to vote against the intent of the proposals. Also voting FOR, Halliday concurred, stating that the case was not quite perfect, but the outcome from the proposals would be correct. Krell, who ABSTAINED in the second round of voting, commented in the first round of voting that he felt the case had formal inconsistencies such as

missing the fixation of a neotype in paragraph 13, but that it was a valid case, thus he was willing to vote for unnecessary but beneficial proposals. During the second round of voting he emphasised his discomfort with the proposals. Ng, voting AGAINST, also did not see the necessity for a vote as the neotype designation was valid and the lectotype designation was faulty anyway. Brothers, voting AGAINST, said that if, as was stated, the 'neotype designation' by Shikama (1966) was deficient, it could not be considered a 'type fixation'. The 'lectotype designation' of Inuzuka (2005) was automatically invalid since the specimen at issue was not a syntype. Action by the Commission with respect to proposal 1) was therefore not possible. Since the other proposals were consequential, they would also fall away. A simple remedy would be to properly designate the desired specimen as the neotype, which would not require the intervention of the Commission. Rosenberg, voting AGAINST, agreed that action by the Commission was not needed; the supposed lectotype had no standing, so the authors were free to validate the neotype designation by providing the missing information. Kottelat said he voted AGAINST because there was no need for a ruling by the Commission in this case as the lectotype had no nomenclatural status since it was not part of the type series and the neotype designation was valid. Grygier, voting AGAINST, said that although the intention of the proposals was fine and he did not want to vote against them, he felt that the formulation was faulty. He said that proposal 1(b) was not a plenary power matter, although phrased as such, as it only sought confirmation of what was already true under the Code. In addition, acceptance of Shikama's type designation in these proposals was only implicit, not explicit, and an explicit proposal confirming the mentioned specimen as the neotype should have been included. Kullander, voting AGAINST, said that paragraphs 6 and 7 indicated that the lectotypification was not sufficiently underbuilt. He preferred to return to this issue when one could get a clear explanation of whether the lectotype was part of the original series or not.

No names are placed on Official Lists or Indexes and the issue is left open for subsequent workers to follow the precepts of the Code or to make new proposals to the Commission.

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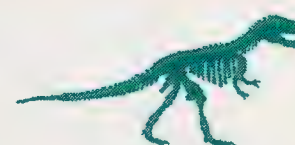
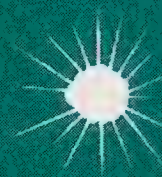
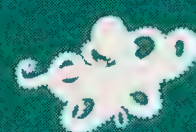
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The Bulletin of Zoological Nomenclature

The Official Periodical of the
International Commission
on Zoological Nomenclature



ICZN

THE BULLETIN OF ZOOLOGICAL NOMENCLATURE

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Cromwell Road

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(Tel. +44 207 942 5653; e-mail: iczn@nhm.ac.uk)

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Cover image: *Portunus pelagicus* (L.), the blue swimming crab native to Indo-Pacific waters, Plate 49 from Zoological drawings by F.L. Bauer (© The Natural History Museum, London, 1997, housed in the collection of the General and Zoology Library).

BULLETIN OF ZOOLOGICAL NOMENCLATURE

Volume 66, part 4 (pp. 297–394)

18 December 2009

Notices

(1) Applications and correspondence relating to applications to the Commission should be sent to the Executive Secretary at the address given on the inside of the front cover and on the Commission website. English is the official language of the *Bulletin*. Please take careful note of instructions to authors (present in a one or two page form in each volume and available online at <http://www.iczn.org/guidelines.html>) as incorrectly formatted applications will be returned to authors for revision. The Commission's Secretariat will answer general nomenclatural (as opposed to purely taxonomic) enquiries and assist with the formulation of applications and, as far as it can, check the main nomenclatural references in applications. Correspondence should be sent by e-mail to 'iczn@nhm.ac.uk' where possible.

(2) The Commission votes on applications eight months after they have been published, although this period is normally extended to enable comments to be submitted. Comments for publication relating to applications (either in support or against, or offering alternative solutions) should be submitted as soon as possible. Comments may be edited (see instructions for submission of comments at http://www.iczn.org/Instructions_for_comments.html).

(3) Requests for help and advice on the Code can be made direct to the Commission and other interested parties via the Internet. Membership of the Commission's Discussion List is free of charge. You can subscribe and find out more about the list at <http://list.afriherp.org/mailman/listinfo/iczn-list>.

(4) The Commission also welcomes the submission of general-interest articles on nomenclatural themes or nomenclatural notes on particular issues. These may deal with taxonomy, but should be mainly nomenclatural in content. Articles and notes should be sent to the Executive Secretary.

New applications to the Commission

The following new applications have been received since the last issue of the *Bulletin* (volume 66, part 3, 30 September 2009) went to press. Under Article 82 of the Code, the existing usage of names in the applications is to be maintained until the Commission's rulings on the applications (the Opinions) have been published.

CASE 3504: *Onthophagus rugulosus* Harold, 1886 (Insecta, Coleoptera, SCARABAEIDAE): proposed conservation of the specific name. F. Krell.

CASE 3505: *Melitta annularis* Kirby, 1802 (currently *Hylaeus annularis*; Insecta, Hymenoptera): proposed suppression of the specific name. P. Westrich & M. Edwards.

Case 3506: *Allosaurus* Marsh, 1877 (Dinosauria, Theropoda): proposed conservation of usage by designation of a neotype for its type species *Allosaurus fragilis* Marsh, 1977. G.S. Paul & K. Carpenter.

Case 3507: *Phylloporina* Ulrich in Foerste, 1887 (Bryozoa, Fenestrata, Phylloporinina): proposed designation of *Retepora trentonensis* Nicholson, 1875 as the type species. F.K. McKinney & P.N. Wyse Jackson.

Unifying nomenclature: ZooBank and Global Names Usage Bank

Taxon names are central to realising the vision of globally interconnected data, including museum specimens, literature, images, video, ecological datasets and other databases. Taxon names are the ‘glue’ that binds most biodiversity information. Unfortunately, much of this information is labelled by taxon names that are little more than text strings, fraught with problems of abbreviations, alternate orthographies, homonyms, novel combinations of binomials and trinomials, and other ambiguities that make it difficult to interpret the intended meaning of the name. The challenge of interconnecting biodiversity data is to establish links between these text-string taxon names and a system of interlinked and unambiguously defined nomenclatural acts. To address this challenge, several organisations have joined forces to develop the *Global Names Architecture* (GNA). With guidance from the GNA Advisory Panel and initial support from GBIF, EoL and NBII, the GNA will include components designed to bridge the gap between text-string taxon names and well-documented nomenclatural acts. One component, the *Global Names Usage Bank* (GNUB), is intended as a public-domain repository for ‘taxon-name usage instances’; an index of the millions of times taxon names are treated in the form of citable documentation (primarily published literature). A subset of these usage instances represents Code-governed nomenclatural acts or taxon concept circumscriptions, and many more are labels to other biological data (e.g. specimens, ecological data and distribution information). In a sense, taxon-name usages (TNU) represent the ‘least-common denominator’ among all nomenclatural Codes, as well as (ultimately) all of biodiversity information.

Development of GNUB has important implications for the implementation of ZooBank. ZooBank was originally conceived as a ‘database’ of nomenclatural acts, associated publications and other relevant information. Our revised vision positions it as a service that operates on the GNUB (with a secure copy maintained by the ICZN). ZooBank would identify specific records within GNUB that constitute ICZN-governed nomenclatural acts (and associated published works), provide a mechanism for validating the accuracy of the contained information and ‘certify’ each in terms of Code compliance. As the Official Registry of the ICZN, this ZooBank service will be vital to (1) representing scientific names correctly and (2) cross-linking and transforming incorrectly rendered scientific names into their Code-correct forms. This would be a major step towards a technological unification of biological nomenclature.

R. Pyle (Commissioner) & E. Michel (Executive Secretary)

INTERNATIONAL TRUST FOR ZOOLOGICAL NOMENCLATURE

Financial Report for the year 2008

The main work of the Commission during the year was on applications from zoologists in 16 countries to resolve problems of zoological nomenclature. These were published in the *Bulletin of Zoological Nomenclature*, together with Opinions (rulings) made by the Commission on other cases. Further applications were under consideration. Advice was given by the Commission's Secretariat in response to a large number of enquiries on matters of nomenclature from zoologists worldwide.

Total income received by the Trust consisted of £31,118 for all publications produced by the Commission, £29,692 from appeal and general donations, £9,054 in bank interest and investment income, and £17,986 capital gain on the sale of investments, bringing the total income for the year to £87,850.

Expenditure in 2008 was £100,644 on salaries and fees of the Secretariat of the Commission, £4,102 on appeal expenditure and ZooBank travel, £9,955 for printing the *Bulletin of Zoological Nomenclature* and for the distribution of all publications, and £7,530 for office expenses and depreciation of office equipment, bringing the total expenditure to £122,231.

The Secretariat of the Commission was again housed in the Natural History Museum, London, whom we thank for their continuing support. The Trust wishes to express its thanks to all the donors listed below who have contributed to the continuation of its work during the year for the international zoological and palaeontological community.

Donations were received from:

Canadian Society of Zoologists
Coleopterists' Society
P. & L. Cooke Endowment
The Earl of Cranbrook
Ichthyological Society of Japan
Internationaler Entomologischer Verein
Kong. Danske Videnskabernes Selskab Copenhagen
Pan-European Species-directories Infrastructure (PESI)
Wellcome Trust

P.L. Forey, Secretary and Managing Director

INTERNATIONAL TRUST FOR ZOOLOGICAL NOMENCLATURE
INCOME AND EXPENDITURE ACCOUNT FOR THE YEAR ENDED
31 DECEMBER 2008

Income**SALE OF PUBLICATIONS**

Bulletin of Zoological Nomenclature	£27,928
International Code of Zoological Nomenclature	2,811
Official Lists and Indexes	314
Centenary History	65

31,118

GRANTS AND DONATIONS

29,692

INTEREST RECEIVED

507

INVESTMENT INCOME

8,547

CAPITAL GAIN ON SALE OF INVESTMENTS

17,986

87,850

Expenditure**SALARIES, NATIONAL INSURANCE AND FEES**

100,644

OFFICE EXPENSES

1,261

**PRINTING OF BULLETIN AND DISTRIBUTION OF
PUBLICATIONS**

9,955

APPEAL EXPENDITURE INCLUDING TRAVEL

4,102

DEPRECIATION OF OFFICE EQUIPMENT

6,269

122,231

DEFICIT FOR THE YEAR CARRIED TO BALANCE SHEET

£34,381

Contributions to the Discussion on Electronic Publication III

Introduction

This is the third instalment of comments on the ICZN proposed amendment on electronic-only publication, which would allow publication of nomenclatural acts on exclusively electronic media to be valid and available. The 30th General Assembly of the IUBS, in Capetown on 11 October 2009, approved the proposals, as outlined in the following resolution:

Resolution: Proposed amendment of the International Code of Zoological Nomenclature

This General Assembly of the IUBS approves the principles underlying the proposed amendment of the International Code of Zoological Nomenclature to expand and refine methods of publication, as published in *Zootaxa* (1908: 57–67) on 9 October 2008, recognising that the final wording of the amendment will require formal approval by the International Commission on Zoological Nomenclature after consideration of the input received from the zoological community over the past year. If approved by the Commission, the final wording of the amendment will be referred to the Officers of the IUBS for their endorsement, and for subsequent implementation.

The proposed amendment is available in the BZN 65: 265–275, several other sources, and online at http://www.iczn.org/electronic_publication.html. We have sought input from all stakeholders in this process, including taxonomists, publishers, archivists, database experts and the wide range of users of nomenclatural information.

The date for the Commission's vote has not yet been set, thus we are not certain whether there will be a further opportunity for input through the BZN, however we encourage continued debate through listservers (e.g. ICZN listserver (<http://list.afriherp.org/mailman/listinfo/iczn-list>) and Taxacom (<http://mailman.nhm.ku.edu/mailman/listinfo/taxacom>)) and the various journals that have published the proposed amendment.

Ellinor Michel
Svetlana Nikolaeva
Natalie Dale-Skey
Steve Tracey

Comments on the ICZN proposed amendment on electronic-only publications

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My comments were first drafted following two days of thought-provoking discussions on harmonisation of the major Codes of Nomenclature, at a meeting held at the Natural History Museum in June 2009, in which I served as the representative of the International Committee in the Systematics of Prokaryotes (ICSP) and the *International Code of Prokaryotic Nomenclature* (ICPN). Overall, I found the proposed amendments to be exceptionally well thought out and am largely in agreement with the authors of the proposal. The observations I offer here are based on past experience with the ICPN and trends on electronic publishing.

In the abstract, I note that there is mention about difficulties in determining if an article was actually published, and when. While this may be problematic for publications with very small print runs, those that carry an ISSN and a DOI can be verified independently based on the date in which registration of the identifier occurred. In the case of DOI (or Handles), the Handle records can be interrogated directly. While this works reasonably well going forward, there may be some problems as legacy content is added into these global systems as a result of large-scale digitisation efforts. I am aware of at least one instance in which a large number of DOIs were issued to back content, but were not registered at the time the content was published on the Internet. Even with the best designed systems, there are chances that human errors result in problems that are unpredictable and difficult to fix.

Although I cannot state with absolute certainty that electronic publication does not cause difficulties in some parts of the world, I think that much of the discussion on the subject is centered more on perception about the developing world rather than statement of fact. We recently looked at this issue in response to comments we received from the Government of Sweden on a white paper that was commissioned by the Secretariat of the UN Convention on Biological Diversity (UNEP/CBD/WG-ABS/7/INF/2). We found that Internet availability statistics (including those published by UNESCO) show that accessibility is growing in all parts of the world, with the highest rates of increase occurring in Africa and Latin America. While coverage may be limited in some places, one could argue that access to print publications is as well. In fact, print publication is nothing more than a way of packaging and distributing information. Publishers held a monopoly on this for a long time because publishing was a capital intensive business. That is not the case any more. The Public Knowledge Project provides some indication as to how quickly our colleagues in developing nations are adopting electronic publishing as means of distributing their own works.

I find the point in the introduction about vanishing paper archives similarly questionable when examined in light of what is known about citation statistics in both the taxonomic and general scientific, technical and medical (STM) literature. Most of the biological names in use are cited only once or a few times in the literature,

and oftentimes those citations tend to be self-citation or citation limited to a small group of collaborators or competitors. This pattern follows the general trend that occurs in the STM literature. Likewise, most of the names that are cited appear in the Western literature (historically) because of its wider availability. That availability is a function of distribution trends by publishers, language barriers, and professional biases. These all have some bearing on the argument that there are relatively few instances in which no known copies exist. Without knowing *a priori* the number of names proposed, the references in which those proposals were made, and which individuals or institutions acquired those publications, there is no way to establish whether or not a publication still exists. All that exists is a small amount of anecdotal information.

With regard to the discussion on PDFs (proposed Article 8.1.3.2), this is an attractive target format, but it is questionable as to whether or not this is the optimum format for archival purposes. Backward compatibility is problematic and current versions of PDF readers are unlikely to handle versions prior to Version 2.0 (Adobe's current version is Version 9.0). Perhaps a better solution is to opt for XML output based on a version of the NLM DTD as that is widely accepted by a number of publishers. As all XML is in ASCII (but supports a variety of character encodings, including Unicode), it will remain readable, long into the future. Style sheets used for rendering XML are also in ASCII, along with the XSL and XSLT used in the transformation process. The NLM also has an archive version of the DTD. Since the vast majority of STM publishers in the US now comply with the requirement of posting acceptable XML to PubMedCentral, this seems to be a reasonable approach for other communities to follow. The other advantage of XML is that it can contain a wealth of metadata in the file header that can be readily extracted, and can include various publisher IDs (ISSN, DOI, Pubmed ID), publication dates, and could include a checksum to verify that the article has not been tampered with post-publication.

The issue of archiving supplementary data (including graphics used in the publication) remains a problem, especially in some fields such as genomics, where sequence trace files can be measured in gigabytes to terabytes. Graphic files can also be problematic, both from the perspective of size, file types, and rendering software. While images can be incorporated into a PDF file during creation, the question about backwards compatibility is unchanged. If one relies entirely on XML files, the image files must be held by the archive as these are merged into the human readable formats (e.g. MTML, PDF) during transformation.

I have some problems with the delay in submitting content to an archive (dark or otherwise) (Proposed Article 8.5.3.3). I suspect that this approach is likely to cause some difficulties in the future and the Commission could be in the position of ruling on names that were included in one or more publications but were not archived by the publisher. While such failures could work against publishers in the future (because authors would be reluctant to submit to those journals), it would punish the current authors for actions that they themselves had no part in. It seems that the more prudent approach would be to require publishers to agree to submit to an archive at the time of publication, with independent verification to both the author and ICZN.

Proposed Article 10.9.2.1 could become problematic over time as it requires an Internet address. Internet addresses are not permanent, nor are domain names. An

alternative approach should be considered that is consistent with current publishing standards. An obvious approach would be to incorporate this information into the DOI record for each article that is published.

Proposed Article 10.9.4 is perplexing. While I can appreciate the political complications of registration, it makes no sense to register only those names that are published electronically. It fragments the record and will only lead to further confusion on the part of the end-users of zoological nomenclature. Why not simply register each nomenclatural event at the time of publication?

Proposed Article 78.2.4 includes language that allows the Commission to establish and maintain an official register of names. As written, the Commission could also cease to maintain such a register at some point in the future. As an outsider, it seems to me that the Commission needs to either decide to take on this task in an official and responsible manner, using proven technology, or suffer the consequences of allowing others to take on the task outside of their control.

Proposed Article 8.1.3.2. PDFs still provide the best option for fixed content, including pagination. PDFs also address the issue of incorporating images directly into the content, and can be locked, signed and certified to insure that changes have not been introduced post-production, and can include some metadata. The issue with PDF files is, and will be, backward compatibility in the future.

Proposed Article 8.4.1. The issue of toner and paper quality has not been adequately addressed. I am unaware of any studies that have been done regarding archiving material produced by this method. This could become problematic in the future, especially in tropical countries.

Article 8.5. The issue of archiving content in XML format should be discussed. This approach is already in widespread use by STM publishers, as noted above. The Commission should also consider redundancy in archives as some will undoubtedly fail to receive adequate financial support to be sustainable in the future.

Recommendations for Article 8 provide a mechanism for an author to submit an electronic article to an archive, but do not provide any guidance as to the nature of the archive or the time constraints in the event that a publisher either chooses not to do so or fails to do so. The language in the article tends to weaken the overall importance of both the registration process and the value of electronic publication. I am mystified as to the reasoning why the Commission would support a system in which only some nomenclatural events would have to be registered while others would not. What possible purpose does this serve? Who benefits from such a system?

References

ICZN. 2008. Proposed amendment of Articles 8, 9, 10, 21 and 78 of the International Code of Zoological Nomenclature to expand and refine methods of publication. *Bulletin of Zoological Nomenclature*, **65**: 265–275.

Proposed Articles discussed above (text in italics indicates proposed changes, text in normal font indicates existing articles of the Code):

Article 8. What constitutes published work. A work is to be regarded as published for the purposes of zoological nomenclature if it complies with the requirements of this Article and is not excluded by the provisions of Article 9.

8.1. Criteria to be met. A work must satisfy the following criteria:

- 8.1.1. it must be issued for the purpose of providing a public and permanent scientific record,
- 8.1.2. it must be obtainable, when first issued, free of charge or by purchase, and
- 8.1.3. it must have been produced in an edition containing simultaneously obtainable copies by a method that assures
 - 8.1.3.1. numerous identical and durable copies (*see Article 8.4*), or
 - 8.1.3.2. widely accessible electronic copies with fixed content and format (e.g. PDF/A, ISO Standard 19005–1:2005) (*see Article 8.5*).
- 8.4.1. *Works printed on paper.* After 2009, the only acceptable means of producing physical copies is by printing on paper using ink or toner.
- 8.5. *Works issued and distributed electronically.* To be considered published, a work issued and distributed electronically must
 - 8.5.1. have been issued after 2009,
 - 8.5.2. state the date of publication in the work itself, and
 - 8.5.3. be archived with an organization other than the publisher in a manner compliant with ISO standard 14721:2003 for an Open Archive Information System (OAIS), or the successors to that standard. (For documentation of the location of the archive, *see Article 10.9.2.1.*)
 - 8.5.3.1. The archiving organization's website must provide a means to determine which works are contained in the archive.
 - 8.5.3.2. The archiving organization must have permanent or irrevocable license to make the work accessible should the publisher no longer do so.
 - 8.5.3.3. If it is found that the work was not deposited in an archive within one year after the work's stated date of publication, or that after the publisher or its successor no longer supports distribution of a work it cannot be recovered from an archive, the case must be referred to the Commission for a ruling on the availability of any names and nomenclatural acts contained in the work.
- 10.9.2. at least the following information must be recorded in the OFFICIAL REGISTER:
 - 10.9.2.1. for the name of a taxon at any rank, sufficient bibliographic information to identify the work in which the name is proposed, and the name and Internet address of the archiving organization,
- 10.9.4. Names and nomenclatural acts published on paper may be registered voluntarily and retrospectively; such registration does not affect their availability.
- 78.2.4. The Commission may establish and maintain an OFFICIAL REGISTER OF ZOOLOGICAL NOMENCLATURE, to record essential information about names and nomenclatural acts. The OFFICIAL REGISTER may be maintained in electronic or paper form. The OFFICIAL LISTS and OFFICIAL INDEXES may be maintained in the OFFICIAL REGISTER.

(2) Cristian R. Altaba

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It is clear that a tighter ruling on publication methods in the proposed amendment will be helpful. Whether abandoning the CD-ROM support beginning in 2010 will reduce debate about validity of publications in the future is unclear to me. Vandalism in nomenclature is a matter of personal attitude, and quite independent of the medium assaulted.

In addition, it is becoming clear that laser disks are not as reliable for long-term storage as once thought, but this can also be said of many paper and electronic publications. The proposed limitation of a CD-ROM or DVD as published for the

purposes of zoological nomenclature only to the period between 2000 and 2009 (proposed Article 8.4.2.1) is a reasonable compromise between improving technologies and the deeply historical nature of zoological nomenclature. Likewise, the proposed wording for Article 8.4.2.2 is beneficial, because it solves potential problems arising from factors beyond authors and publishers acting in good faith. This is timely indeed, given that cases of taxonomic vandalism affect the reputation of zoological nomenclature and threaten its central role in comparative biology (e.g. Anonymous, 2007; Daniels, 2008; Altaba, 2009).

It is also clear that paper and PDF versions of a publication are nowadays frequently released simultaneously, potentially creating problems with the perceived compliance to the Code. Thus, the wording of the proposed Article 21.9 is adequate but should be modified in order to encompass situations where both the paper copies and the PDF version are available at the same time. I propose the following wording:

21.9. Works issued on paper and electronically. A name or nomenclatural act published in a work issued in both print and electronic editions is available from the one that first fulfils the relevant criteria of availability. In case both editions are available simultaneously, the physical (paper) version is to be considered available first.

Most importantly, controversies in nomenclatural priority highlight the urgent need for a centralised Official Register (proposed Article 10.9) to avoid unnecessary, time-consuming debate.

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(3) Brian Taylor

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Without wishing to be unpleasant, I find the attitude expressed by Francisco Welter-Schultes and co-signatories archaic and retrograde. It also seems highly selfish, with almost all the signatories coming from ‘Western’, i.e. rich, countries. Having lived and worked in some of the more remote areas of the world and now having a cohort of enthusiastic e-mail correspondents, I can testify to the success of the internet in bringing taxonomy out of the closed world of the major museums and libraries. For my much appreciated website *The Ants of sub-Saharan Africa*, I have been able to access some 461 papers of an essentially taxonomic nature, the earliest being published in 1767 and the most recent in 2008. I have developed the website with very little other than personal funding and it was the tremendous effort of Donat Agosti and colleagues in making the source papers available on <http://www.antbase.org/> that made my archival research possible. The fact mentioned by

Welter-Schultes that almost all those files, in PDF format, are copies from printed books is no more than a fact of history. It does not have value as a demerit of the electronic medium.

Whilst it surely is true that 'Long-term preservation of electronic information continuously requires high financial inputs' the same applies, probably at even higher cost, for the maintenance of a major library storing printed materials under the optimum conditions of security and climate. As a member of the Royal Entomological Society Library Committee, I have some insight into such matters. They refer also to a requirement for a minimum of 100 paper copies. I cannot envisage there being more than perhaps 20 major libraries that would or could conserve paper copies for posterity. Further, a problem facing many, if not all, academic libraries is the cost of subscribing to ever more expensive journals.

I agree with David E. Hill that peer review arguably is unnecessary. Revisionary studies by taxonomists always have excised errors simply by synonymy and will continue to do so. He urges the merits of digital photography to back up, indeed to form an integral and essential part of, descriptions. There is a problem here with say, *Zootaxa*, where the online version is accompanied by a print version. The journal itself has limited funding and cannot support colour photographs in the print version and, at present, appears to limit the number of photographs.

To go back to my earlier comment on rich countries, there is an irony in Michael P. Taylor's contribution where he wrote of a nominal fee of \$10 by mail order. For anyone not employed under a relatively well financed grant or whatever, a number of such \$10 fees might be prohibitive and many journals charge far more than that for copies of individual papers, even when accessed online.

On a technical note much reference is made to the PDF format. The true ubiquitous text format is the RTF, with the proprietary Microsoft Word not far behind. The images can be stored in the standard JPG or JPEG formats. It is the web page, however, that provides the most useful format for taxonomy and the, clearly oft forgotten, users of taxonomic information, such as ecologists and population biologists. Links to and from keys and, so, to comparable species provide an enormously effective tool and images can be provided on the page without need to flick back and forth through a paper or book.

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I fully support the comments of Francisco Welter-Schultes and would like to add the following on the problem of an Official Register. For 12 years I have been involved in writing a Catalogue of Palaearctic Coleoptera, in collaboration with 160 specialists. This project covers some 100,000 valid taxa and about 200,000 available taxonomic names. In addition, primary references are given to each of the genus- and species-group names (at the moment, 5 volumes with some 4000 printed pages are published; one following volume may yet be completed this year).

While working on this project we met a large number of difficulties; among the more common ones were poor understanding of nomenclature (professionals

included), poor knowledge of published sources and inconsistencies at all levels. Alpha-taxonomists, who are responsible for recognition of essential parts of the diversity of life, commonly have never consulted the Code and have poor knowledge of Opinions. For instance, names published as varieties of subspecies or as aberrations are used for/as valid taxa and genus-group names were published even in the 80s without having been fixed by type species (again by professionals).

The poor knowledge of the Code was illustrated in an interesting way by French phylocodists (again professional zoologists) who ignored the fact that names above family-group are not regulated, and believed that types define the contents of taxa.

Experience suggests that if we introduce an Official Register, some workers will base their work on it while others may ignore it. Thus, in subsequent work on one and the same taxon, some authors will use only registered names while others may use also the unregistered ones. It is perfectly predictable that an Official Register, however useful it may seem to be, will result in a major mess in taxonomy.

Alpha-taxonomy is not adequately supported in most countries. One of the secondary effects of the present situation may be seen in the fact that many taxonomists in Europe work with ideas of the past typologically and cannot even distinguish available and unavailable names. Misspellings are commonly considered as synonyms. The same is true for nomina nuda.

Efforts should be redirected to other issues than Official Registers and similar ambitious projects. To improve the actual situation in taxonomy we need to deal with very different issues such as the reintroduction of systematics (with alpha- and beta-taxonomy and nomenclature) in universities, where it has been downsized in the last 30 years, and a re-evaluation of the role of natural history museums in which popularisation has become one of the most important criteria.

Archiving & records management perspectives on electronic-only publications for nomenclature

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I fully support the establishment of taxonomic names in electronic-only publications and would like to make some recommendations based on my professional experience in archival sciences, as a Scholarly Communications Librarian. The proposed amendment to the Code needs only to be explicit in what is required of authors and publishers for electronic-only publication, and all contingencies can be covered. In this generation we have one foot in the world of paper and the other now firmly planted in the electronic realm. This special position will be relatively short-lived, and we would be doing a great service to our successors if we were to establish some viable guidelines for authoritative naming of taxonomic units early on in this transition to digital media.

My recommendations are as follows:

As discussed in the draft proposed amendment there must be a requirement that electronic-only publications containing nomenclatural acts have archival integrity.

This means they should be accredited by, authenticated by, or registered with, services such as Portico (<http://www.portico.org>), or be a part of large taxonomic archiving efforts such as the Biodiversity Heritage Library (<http://www.biodiversitylibrary.org/About.aspx>). To determine whether to publish in any given publication, authors may ask editors if the publication participates in any such initiatives. If the publication does not participate in an established archival initiative, the author may inquire if the publication is available in its entirety in an institutional repository. Ultimately, the author must decide whether he or she believes the publication meets this requirement.

The amendment should recommend that authors submit their work to publishers who do not restrict access to the published work. Authors should be allowed to freely post the final, published version of the article or chapter in any scholarly repository and on the author's personal website (with no restrictions other than a possible short embargo, and preferably with no fees attached to this agreement). It is important that authors should be able to post the final, published version, in its exact original form, not merely pre-prints. Without this caveat, myriads of issues arise regarding authoritativeness of name due to availability of various versions of an article. Pre-print versions of an article are manuscripts, and therefore are not authoritative in terms of name establishment.

Akin to the U.S. National Institutes of Health (NIH) initiative (requiring deposition of articles in PubMed Central by funded authors), authors who publish taxonomic names in electronic-only format should be required by the Code to have their published, peer-reviewed paper uploaded in a scholarly repository that is freely accessible to anyone with a web connection.

To avoid confusion as grey literature becomes more readily available online, I recommend wider use of disclaimers. With theses and dissertations increasingly being made freely available online, as well as abstracts and conference proceedings, authors should be directed to clearly disclaim first-time use of names in all non-peer reviewed, manuscript-like, grey literature-type online publications. Examples of proposed wording should be provided on the ICZN website so that authors and editors can unambiguously and consistently incorporate appropriate language into such works. In addition, libraries can begin to prominently display blanket disclaimers on their repository websites regarding the non-availability of names in posted documents that might be mistaken for available published works.

Until another format emerges supplanting it, suggesting that articles be deposited in PDF is a good idea, to ensure the accuracy of presentation in every viewing instance of a document, article or chapter.

CD-ROMs and DVDs should be disallowed as mechanisms of valid publication. Their archival stability is uncertain. The content of these media is easily converted to PDF and uploaded in a repository.

There should be no requirement for authors who publish names in electronic-only publications to also somehow place a paper version of the article in any number of libraries. However, authors should be required to state explicitly in the publication that they have abided by the requirements of the Code for depositing of names and to state which electronic repositories the article will reside in, in perpetuity.

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The proposed amendment states (in summary) that: 'Electronic-only publications should be allowed, if mechanisms can be found that give reasonable assurance of the long-term accessibility of the information they contain.' From a records management perspective, you cannot guarantee long term accessibility, you should guarantee digital continuity and put plans in place to deliver. I doubt you will find a mechanism until you define a time period and continuity plan.

Digital obsolescence also needs to be considered for both historical content and formats and for the future. CD-ROM is hardly a durable medium. Neither is paper.

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I think that one thing no scientific (or any other) community can risk is failure to make best use of the communications technologies that we now have at our command. Online publishing provides opportunities for very broad dissemination of new ideas and results findings, providing increased opportunities for global peer review.

Here in Australia, the National Library of Australia has been collecting significant Australian online publications since 1996. The National Library of Australia (and the International Library Community) has also been doing work in relation to legal deposit, and its application to online publications. Collecting libraries develop their selection guidelines attempting to identify items that best support their mandate and meet the needs of their user community. Having passed this selection review the collected e-publication is likely to have some level of 'substance'.

If the Code permitted new animal names to be published in online publications that could demonstrate that they were subject to deposit with a national library or other established collecting body, this should ensure longer-term accessibility of the publication as the collecting institution would address the issue of long-term preservation formats and storage. This would be likely, of course, to exclude the small 'self-published' e-publications, some with only a very short life-span, and perhaps reduce the risk of accepting taxonomy published within publications with less rigorous editorial review or publishing standards.

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Australia is a Federation so there are jurisdictional differences across the country but the Commonwealth and the majority of States have, over the last decade or so, enacted uniform Evidence Acts which remove the medieval definitions of documents and give equivalence to digital and physical records. These have been supplemented by Electronic Transaction Acts which amend all current laws in those jurisdictions by stating that electronic communication constitutes written communication and that recording something in a digital medium is recording it in writing. That is a simplistic description but the point of the two Acts taken together is to abolish, at least in most of Australia, the distinction you currently make between printed and digital only journals. Whether this will spread internationally is a moot point. Lawyers seem to regard it as the end of civilisation as we know it and one State, South Australia, has rejected the uniform Evidence Act.

I would also support the exclusion of anything published only on CD-ROM or DVD as these are not permanent media.

Case 3493***Haliplanella* Hand, 1956 (Anthozoa, Actiniaria): proposed conservation by suppression of *Haliplanella* Treadwell, 1943 (Polychaeta)**

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Abstract. The purpose of this application, under Article 23.9.3 of the Code, is to conserve the generic name *Haliplanella* Hand, 1956 for a widespread sea anemone by suppressing the senior name *Haliplanella* Treadwell, 1943 that was first, through an error, used for a group of polychaete annelids, was later put into synonymy, and is now no longer used.

Keywords. Nomenclature; taxonomy; Cnidaria; Anthozoa; Actiniaria; Annelida; *Haliplanella*; *Haliplanella lineata*; sea anemones; worldwide.

1. Homonymy exists between the genus-group names *Haliplanella* Treadwell, 1943 (Polychaeta) and *Haliplanella* Hand, 1956 (Anthozoa). The senior name has generally been omitted from lists of generic names and has long been relegated to synonymy, whereas the junior name has enjoyed currency and forms the basis of a family-group name. In our opinion stability would best be served by suppressing the senior homonym.

2. This situation was first brought to the attention of the International Commission on Zoological Nomenclature in 1977 as Case 2192 by Dunn & Hand (1977) but was never voted on. Comments on the case by den Hartog (1978) and Williams (1978) were published in the Bulletin of Zoological Nomenclature and responded to by Dunn & Hand (1978). In recent correspondence with the Secretary of the Commission it was established that Fautin (who had written under the name Dunn in the 1977 appeal) had agreed to hold back this application pending publication of 'den Hartog's new taxonomic evidence'. However this evidence was never published and den Hartog died in 2000. It seems that taxonomic concerns (discussed below) prompted the Commission to delay the voting on this Case, but the nomenclatural issue remains and should be resolved. Considering the length of time elapsed and the

many developments since the original appeal, the Secretary advised that a new application be formulated rather than resuscitating Case 2192.

3. The genus-group name *Haliplanella* was first published by Treadwell (1943), p. 32 for a pelagic marine polychaete. He provided a short summary of the differences between the genera *Haliplanes* and *Haliplanella*, attributing both genera and the differential diagnosis to Reibisch (1895). The use of the name *Haliplanella* was clearly an error because it does not appear in any of Reibisch's published work, although the pages cited by Treadwell do contain descriptions of the genus *Haliplanes* and of the species *Haliplanes gracilis*. In the same paper, Treadwell (1943, p. 32) described a new species, *Haliplanella pacifica* (holotype 20079, United States National Museum), whereby the name *Haliplanella* Treadwell, 1943 met the criteria of availability, with *H. pacifica* the type species by monotypy. Although Treadwell has been recognised as author of the genus (Dales, 1957; Hartman, 1956, 1959; Ushakov, 1972), *Haliplanella* did not appear in Volume 5 of *Nomenclator Zoologicus* (Neave, 1950), probably because it had not been intended as a new name by Treadwell (1943).

4. The sea anemone in question, variously known as *Haliplanella luciae*, *H. lineata*, *Diadumene luciae* or *D. lineata*, is the most widespread species of anemone in the world (e.g. Seaton, 1985; Fautin et al., in press), having apparently been dispersed in part through human agency (Zabin et al., 2004). Its distribution is probably attributable to its ability to tolerate extremes of salinity and temperature better than most anemones, and its ability to reproduce asexually (e.g. Minasian, 1982). Because of these broad tolerances, and the associated ease of keeping animals in the laboratory, it has been the subject of much physiological and ecological research (e.g. Williams, 1968, 1972, 1973, 1975; Sassaman & Mangum, 1970, 1973; Kiener, 1971; Atoda, 1973, 1976; Shick, 1976; Mire & Venable, 1999; Watson et al., 2008). Unlike many anemones, it is so morphologically distinctive as to be almost unmistakable, and so the taxonomic issue alluded to above does not relate to the problem of identifying an invasive species. In 1869, Verrill (p. 23) described *Sagartia lineata* based on preserved specimens from Hong Kong that were accompanied by notes on colour, habitat, etc. in life. In 1898, Verrill (p. 393) described *Sagartia luciae* based on live specimens from Woods Hole, Massachusetts, that were discovered by his daughter, Lucy. No type material of either species exists (Fautin, 2008). It is now widely accepted (e.g. Seaton, 1985) that both of Verrill's descriptions refer to a single species. In Verrill's day this might have seemed unlikely, but in the light of what we now know about the distribution and tolerances of the species, it is a reasonable conclusion. In 1956 (pp. 210–211), Hand proposed the genus *Haliplanella* for the sea anemone described as *Sagartia luciae* Verrill, 1898 (pp. 493–494). The species has also been included in other genera (see synonymies in Hand, 1956; Fautin, 2008). At the same time Hand (1956, p. 210) established the monotypical family HALIPLANELLIDAE for *Haliplanella luciae* (Verrill, 1898). Neave (1939) listed 'Haliplanes (pro Haly-Reibisch, 1893) Reibisch, 1894, Pelag. Phyllodoc., 3, 12.—Verm (Polych.)' and the online version notes it is a junior homonym. The subsequent volume of *Nomenclator Zoologicus* (Edwards & Hopwood, 1966, p. 115) listed '*Haliplanella* Hand 1955, *Wasmann J. Biol.* Vol. 13: 210.—Coel.'

5. The first comment by Williams (1978) on the proposal by Dunn & Hand (1977) to suppress the name *Haliplanella* for the polychaete in favour of that for the sea

anemone concerned the date of Hand's description; Dunn & Hand (1978) conceded that the issue of the journal was dated 1955 but appeared in 1956, as documented by a note in the subsequent volume of the journal. The second comment by Williams (1978) as well as those by den Hartog (1978) were taxonomic in nature. den Hartog (1978, p. 74) thought it likely that *Haliplanella* and HALIPLANELLIDAE would 'disappear in synonymy', so the appeal by Dunn & Hand (1977) was moot. Although late in his life Hand eventually came to share that opinion, the name *Diadumene* Stephenson, 1920 (type species by monotypy *Tricnidactis errans* de Oliveira Pires, 1987) has been placed in the family *Haliplanellidae*, which would be invalid under Article 39 unless this application, first made in Case 2192, is granted.

6. Hartman (1956) synonymised *Haliplanella pacifica* Treadwell, 1943 under *Halyplanes gracilis* Reibisch, 1893, the type species of *Halyplanes*. In 1957, Dales synonymised the genus *Halyplanes* under the genus *Maupasia* Viguier, 1886. Thus *Haliplanella pacifica* Treadwell is now considered a junior subjective synonym of *Maupasia gracilis* (Reibisch) (Hartman, 1959; Ushakov, 1972).

7. From 1955 through the latest issue available online, the generic name *Haliplanella* appeared in *Biological Abstracts* once for the polychaete, as an invalid synonym (Hartman, 1956), and 55 times for the anemone, often as a valid name (aside from citations in paragraphs 4 and 5, above, e.g. Minasian & Mariscal, 1979; Watson & Mire, 2004) and also as a synonym (e.g. Molina et al., 2009). Thus, during the past 54 years, the name *Haliplanella* has appeared in the literature for the polychaete only once, as a junior synonym of *Maupasia* Viguier, 1886, while *Haliplanella* is still used for the anemone. Under these circumstances, although the conditions of Article 23.9.1 for automatic maintenance of prevailing usage are not met, we believe that conservation of *Haliplanella* Hand, 1956 will best serve stability of nomenclature and thus refer the matter for a ruling as specified in Article 23.9.3.

8. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to suppress the generic name *Haliplanella* Treadwell, 1943 and all uses of the name for the purposes of both the Principle of Priority and the Principle of Homonymy;
- (2) to place on the Official List of Generic Names in Zoology the name *Haliplanella* Hand, 1956 (gender: feminine), type-species by monotypy *Sagartia luciae* Verrill, 1898 (Anthozoa);
- (3) to place on the Official List of Specific Names in Zoology the name *lineata* Verrill, 1869, as published in the binomen *Sagartia lineata* (senior subjective synonym of the type-species of *Haliplanella* Hand, 1956);
- (4) to place on the Official Index of Rejected and Invalid Generic Names in Zoology the generic name *Haliplanella* Treadwell, 1943 (Polychaeta), as suppressed in (1) above.

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Comments on this case are invited for publication (subject to editing) in the *Bulletin*; they should be sent to the Executive Secretary, I.C.Z.N., c/o Natural History Museum, Cromwell Road, London SW7 5BD, U.K. (e-mail: iczn@nhm.ac.uk).

Case 3481***Crioceris quadripunctata* Olivier, 1808 (currently *Petauristes quadripunctatus*; Insecta, Coleoptera): proposed conservation**

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Abstract. The purpose of this application, under Article 23.9.5 of the Code, is to conserve the name *Petauristes quadripunctatus* (Olivier, 1808) for a common and widespread South Asian beetle originally described as *Crioceris quadripunctata*, and thus a junior primary homonym of *Crioceris quadripunctata* Fabricius, 1801 (currently *Monolepta quadripunctata*).

Keywords. Nomenclature; taxonomy; Insecta; Coleoptera; CHRYSOMELIDAE; *Petauristes*; *Petauristes quadripunctatus*; Southern Asia.

1. Fabricius (1801, p. 460) described the species *Crioceris quadripunctata* from Nova Cambria (= New South Wales). Olivier (1808) referred to it as *Galeruca quadripunctata*, and Blackburn (1896) transferred it to *Monolepta*; it has since then (Weise, 1924; Wilcox, 1973) been listed as *Monolepta quadripunctata*.

2. Olivier (1808, p. 731) described the species *Crioceris quadripunctata*. This species was referred to as *Lema quadripunctata* by Lacordaire (1845), and has since been listed as *Lema quadripunctata* by e.g. Jacoby (1908), Monrós (1960), Kimoto & Gressitt (1979), Medvedev (1985) and Takizawa (1990), as *Bradylema quadripunctata* by Heinze (1941), and as *Lema (Petauristes) quadripunctata* by e.g. Tan et al. (1985), Medvedev (2000) and Mohamedsaid (2004) (*Bradylema* Weise, 1901 is considered a junior synonym of *Petauristes* Latreille, 1829). The species is widely distributed from India and southern China to Indonesia and the Philippines. Lacordaire (1845) listed one synonym, namely *Lema quadrisignata* Boisduval, 1835 (p. 534), described from Australia, and the synonym was mentioned by Clavareau (1913), Monrós (1960) and Kimoto & Gressitt (1979); *Lema quadrisignata* has not been considered a separate species since its original description, but because of its geographic origin, the synonymy ought to be investigated. Heinze (1941, p. 208) described two subspecies, *Bradylema quadripunctata ceylonica* and *Bradylema quadripunctata philippinica*; nobody has raised either of these to species rank. Kimoto & Gressitt (1979) further synonymised the nominal species *Lema subinnotata* Pic, 1929 (p. 15) with *L. quadripunctata*; Pic's species had not been mentioned except in a list by Monrós (1960).

3. The name *quadripunctata*, in combinations *Lema quadripunctata* (Olivier) or *Lema (Petauristes) quadripunctata* (Olivier), is widely used, e.g. by Kimoto (1984), Takizawa (1988), Mohamedsaid (1990), Schmitt (1990), Medvedev (1995) and Reid (1997).

4. *Crioceris quadripunctata* Olivier, 1808 is a junior primary homonym of *Crioceris quadripunctata* Fabricius, 1801, although the two species have not been included in the same genus since the early 19th century. Article 23.9.5 of the Code states that such a case should be referred to the Commission.

5. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to rule that the name *quadripunctata* Olivier, 1808, as published in the binomen *Crioceris quadripunctata*, is not invalid by reason of being a junior primary homonym of *quadripunctata* Fabricius, 1801, as published in the binomen *Crioceris quadripunctata*;
- (2) to place on the Official List of Specific Names in Zoology the following names:
 - (a) *quadripunctata* Fabricius, 1801, as published in the binomen *Crioceris quadripunctata*;
 - (b) *quadripunctata* Olivier, 1808, as published in the binomen *Crioceris quadripunctata*, with the endorsement that it is not invalid by reason of being a junior primary homonym of *quadripunctata* Fabricius, 1801, as published in the binomen *Crioceris quadripunctata*, as ruled in (1) above.

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Case 3489***Chrysomela elongata* Suffrian, 1851 (currently *Oreina elongata*; Insecta, Coleoptera): proposed conservation**

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Abstract. The purpose of this application, under Article 23.9.5 of the Code, is to conserve the use of the well known alpine leaf beetle name *Oreina elongata* (Suffrian, 1851), originally described as *Chrysomela elongata*, and thus a junior primary homonym of *Chrysomela elongata* Linnaeus, 1758, currently known as *Tillus elongatus* (Linnaeus, 1758).

Keywords. Nomenclature; taxonomy; Insecta; Coleoptera; CHRYSOMELIDAE; *Oreina elongata*; *Oreina elongata elongata*; alpine leaf beetle.

1. Linnaeus (1758, p. 377) described a species *Chrysomela elongata*. Fabricius (1775, p. 125) referred to it as *Lagria elongata* and Müller (1776, p. 85) as *Crioceris elongata*. Olivier (1790, part 22, p. 4) included it in his new genus *Tillus*, and there it has remained ever since, in the family CLERIDAE. *Tillus elongatus* is a well known species, as shown by a list of 20 additional references held by the Secretariat. A selection of the more important works mentioning *Tillus elongatus* is given below.

2. Suffrian (1851, p. 146) described a species *Chrysomela elongata*. Kraatz (1859, p. 286) transferred it to the genus *Oreina* Chevrolat, 1837, where it has remained ever since although the species has occasionally been referred to as *Orina* Weise, 1884 or *Chrysochloa* Hope, 1840, e.g. by Lindroth (1960). Bechyné (1958) listed a number of synonyms, some of them with subspecific status, but none of these have been used as the valid name for the species. *Oreina elongata* is also a well known and much studied species (a list of 12 additional references is held by the Secretariat). A selection of the more important works mentioning *Oreina elongata* is given below.

3. *Chrysomela elongata* Suffrian, 1851 is a junior primary homonym of *Chrysomela elongata* Linnaeus, 1758 although the two species have not been considered congeneric after 1899. The combination *Oreina elongata* has been used in numerous papers (Freude et al., 1966; Daccordi & Ruffo, 1976, 1986; Jakob, 1979; Dobler & Rowell-Rahier, 1994; Termonia et al., 2001; Böhme, 2005; Margraf et al., 2005; Röder et al., 2007). The combination *Tillus elongatus* has also been widely used (Richter, 1965; Pope, 1977; Allenspach & Wittmer, 1979; Bercio & Folwaczny, 1979; Burakowski et al., 1986; Telnov et al., 1997; Gerstmeier, 1998; Pérez Moreno & Cabrero Sañudo, 1999; Silfverberg, 2004; Böhme, 2005; Krivolutskaja et al., 2007; Löbl et al., 2007). Article 23.9.5 of the Code states that such a case should be referred to the Commission.

4. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to rule that the name *elongata* Suffrian, 1851, as published in the binomen *Chrysomela elongata*, is not invalid by reason of being a junior primary homonym of *elongata* Linnaeus, 1758, as published in the binomen *Chrysomela elongata*;
- (2) to place on the Official List of Specific Names in Zoology the following names:
 - (a) *elongata* Linnaeus, 1758, as published in the binomen *Chrysomela elongata*;
 - (b) *elongata* Suffrian, 1851, as published in the binomen *Chrysomela elongata*, with the endorsement that it is not invalid by reason of being a junior primary homonym of *elongata* Linnaeus, 1758, as published in the binomen *Chrysomela elongata*, as ruled in (1) above.

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Case 3497***Cyphon palustris* Thomson, 1855 (Insecta, Coleoptera): proposed conservation of the specific name**

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Abstract. The purpose of this application, under Article 23.9.5 of the Code, is to conserve the specific name of the widespread Palaearctic marsh beetle *Cyphon palustris* Thomson, 1855 (SCIRTIDAE), published as a junior primary homonym of *Cyphon palustris* Germar, 1818 (currently *Eubria palustris*, PSEPHENIDAE). Both specific names are in current use and have not been considered congeneric after 1899. It is proposed to conserve the name *Cyphon palustris* Thomson, 1855 by ruling it not to be invalid by reason of being a junior primary homonym.

Keywords. Nomenclature; taxonomy; Insecta; Coleoptera; SCIRTIDAE; PSEPHENIDAE; *Cyphon*; *Eubria*; *Cyphon palustris*; *Eubria palustris*; marsh beetles; water penny beetles; Palaearctis.

1. Germar (1818, p. 238) established the name *Cyphon palustris* for a species of water penny beetle (currently *Eubria palustris*, PSEPHENIDAE) from Germany ('hiesiger Gegend' [=Halle] and Dresden). In the same publication (p. 239) the new genus *Eubria* was conditionally proposed for this species, and it is available from here (Article 11.5.1 of the Code). The transfer to *Eubria* is also available from here (Article 11.9.3.6). The species has a widespread West-Palaearctic distribution (Jäch et al., 2006). Since its description all other authors consistently placed *C. palustris* Germar in the genus *Eubria* Germar (e.g. Audouin, 1824; Westwood, 1838; Stephens, 1839; Laporte de Castelnau, 1840; Redtenbacher, 1849; Bach, 1852; Kiesenwetter, 1860, 1863; Mulsant & Rey, 1865; Tournier, 1868). This practice was undoubtedly supported by the use of *Eubria palustris* in Dejean's catalogues, at that time serving as the world catalogues of Coleoptera (Dejean, 1821, 1833, 1836). The fact that in these works both the specific and generic names were wrongly attributed to Ziegler is irrelevant here. The only citation of the original combination *Cyphon palustris* that could be traced was by Germar himself (Germar, 1822, pl. 3). The widely accepted placement in *Eubria* is reflected by its treatment in the other major catalogues (Gemmingen & Harold, 1869; Reitter, 1906; Pic, 1914; Jäch et al., 2006). A lectotype was designated by Lee & Jäch (1996, p. 40).

2. Thomson (1855, p. 320) used the name *Cyphon palustris* to describe a species of marsh beetle (SCIRTIDAE) from Sweden (Skåne, Arrie). The species has a widespread Palaearctic distribution (Nyholm, 1972). *Cyphon palustris* Thomson was initially treated as a valid species (e.g. Kiesenwetter, 1860; Mulsant & Rey, 1865; Tournier, 1868; Gemminger & Harold, 1869; Redtenbacher, 1874), later as variety or aberration of *C. coarctatus* Paykull, 1799 (e.g. Thomson, 1864; Bourgeois, 1884; Everts,

1903; Reitter, 1911; Pic, 1914; Porta, 1929) or incorrectly as a synonym of *C. coarctatus* (e.g. Fowler, 1890). Nyholm (1955, p. 264) established *C. palustris* Thomson, 1855 to replace *C. coarctatus* auctorum nec Paykull. The oldest available junior synonym of *C. palustris* Thomson, 1855 is *Cyphon fuscicornis* Thomson, 1855 (p. 320) as recognised by Tournier (1868, p. 53) when he synonymised *C. fuscicornis* and *C. coarctatus* auctt. non Paykull (i.e. *C. palustris* Thomson). The synonymy of these two taxa is still considered correct today (e.g. Gemminger & Harold, 1869; Reitter, 1906; Pic, 1914; Nyholm, 1972; Klausnitzer, 2006). A lectotype of *Cyphon palustris* Thomson, 1855 was designated by Nyholm (1972, p. 36).

3. *Cyphon palustris* Thomson, 1855 is a junior primary homonym of *C. palustris* Germar, 1818. Both names are in current use. Since its introduction *C. palustris* Germar has, with one exception (Germar, 1822), consistently been placed in *Eubria*. *Cyphon palustris* Thomson thus meets the requirements of Article 23.9.5 of the Code. Without any further action the name *C. palustris* Thomson for a common and widely distributed Palaearctic species of marsh beetle would be replaced by the long forgotten name *Cyphon fuscicornis* Thomson 1855, which has not been used as valid since the 19th century. This would not be in the interest of nomenclatural stability. A list of 27 references demonstrating the prevailing usage of *C. palustris* Thomson, 1855 is held by the Secretariat.

4. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to rule that the specific name *palustris* Thomson, 1855, as published in the binomen *Cyphon palustris*, is not invalid by reason of being a junior primary homonym of *palustris* Germar, 1818, as published in the binomen *Cyphon palustris*;
- (2) to place on the Official List of Specific Names in Zoology the following names:
 - (a) *palustris* Thomson, 1855, as published in the binomen *Cyphon palustris*, with the endorsement that it is not invalid by reason of being a junior primary homonym of *Cyphon palustris* Germar, 1818, as ruled in (1) above;
 - (b) *palustris* Germar, 1818, as published in the binomen *Cyphon palustris*.

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Case 3491

***Podargus cornutus* Temminck, 1822 (currently *Batrachostomus cornutus*; Aves, PODARGIDAE): proposed conservation of usage of the specific name by designation of a neotype**

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Abstract. The purpose of this application, under Article 75.6 of the Code, is to conserve the accustomed usage of the name *cornutus* Temminck, 1822 for the Sumatran population of the southeast Asian species currently named *Batrachostomus javensis* (Horsfield, 1821) (Aves, PODARGIDAE). To this end, we ask that Temminck's action in proposing the replacement of Horsfield's name *javensis* with *cornutus* be set aside so that *cornutus* does not become a junior objective synonym of *javensis* (type locality in Java) and that the Sumatran specimen which Temminck actually described as *cornutus* be designated as its neotype. This would establish *cornutus* for the Sumatran form for which it has been in use for over 70 years as the subspecific name.

Keywords. Nomenclature; taxonomy; Aves; PODARGIDAE; *Batrachostomus*; *Podargus*; *Batrachostomus javensis*; *Batrachostomus cornutus*; frogmouth; Java; Sumatra.

1. Horsfield (1821a, p. 141) described a new frogmouth from Java, which he named *Podargus javensis*. The specimen considered to be the holotype is extant and present in the collection of the Natural History Museum, Tring, U.K. (Warren, 1966, p. 148). Later, Horsfield (1821b) provided a colour plate of this frogmouth, using the incorrect subsequent spelling *javanensis*. The date of this latter work (November) is to be found in the Day Book of the East India Company Library (Bastin, 1990, p. 75). Bastin's dates have been summarised by Dickinson (in press).

2. Temminck (1822, text to pl. 159), believing he had located a specimen of the same taxon from Sumatra, named this specimen as *Podargus cornutus* and explicitly stated that he was replacing Horsfield's name. It is evident from Temminck's general practice during the period 1820 to 1840 that he disliked the use of local geographic names, especially when he believed they obfuscated wider geographic occurrence. Temminck, in the same text, wrote: 'M. Horsfield a le premier décrit et donné une figure de cet oiseau. Ce naturaliste d'ailleurs très-distingué, a été peu soigneux dans le choix des noms donnés aux espèces nouvelles qu'il fait connaitre; les dénominations d'îles et de contrées peu étendues ont été imposées à presque toutes les espèces de mammifères et d'oiseaux contenus dans ses fascicules. Le Podarge de cet article figuré dans la seconde fascicule, porte aussi le nom de *Javanensis*, que nous avons changé en celui de *Cornutus*, afin d'indiquer un caractère marquant qui distingue cette espèce' [Mr Horsfield first described and included a figure of this bird. This naturalist, otherwise very distinguished, has been less than careful in his choice of names given to the new species he has made known; the names of islands and countries of small extent have been imposed on almost all the species of mammals and birds contained in his works. The frogmouth of this article, figured in the second part, also bears the name *Javanensis* which we have changed to *Cornutus* in order to draw attention to a remarkable character which distinguishes this species]. Temminck then added: 'M. Horsfield n'a pu se procurer qu'un seul individu de cette espèce, très-difficile à découvrir dans les retraites sombres où elle vit cachée pendant le jour, on la trouve à Java, dans les grandes forêts. L'individu du Musée de Paris qui nous a servi de modèle, a été envoyé de Bencoule, dans l'île de Sumatra, par MM. Diard et Duvaucel' [Mr Horsfield was only able to obtain a single individual of this species, which is very hard to discover in the shady retreats where it lives concealed during the day, one finds it in Java in large forests. The individual in the Paris Museum, which has served us as a model, was sent from Bencoolen (now Bengkulu) in the island of Sumatra by Messrs Diard and Duvaucel]. Temminck had his artist depict the specimen that he had found in Paris, collected by Diard and Duvaucel. This specimen is still extant and has been listed, incorrectly, as the holotype of *Podargus cornutus* Temminck by Cleere et al. (2006).

3. Chasen (1935) listed *cornutus* as a synonym of *javensis*, but noted their distinct geographic origins. It fell to Stresemann (1937), in a thorough review of this genus, to conclude that *javensis* and *cornutus* were at least subspecifically distinct. Stresemann (1937) mentioned that the type of *cornutus* was in Paris, although he did not give its details so presumably did not examine it. Nor did he appreciate that *cornutus* Temminck was in effect a new name for *javensis* Horsfield. Only in 1986 did Mees (1986, p. 54) observe that it was Temminck's intention to replace Horsfield's name rather than to describe a new taxon. We concur with this view, although the two populations have been considered distinct since 1937, and point out that without preventative action, Temminck's name, in universal usage since 1937, will have to be treated as a junior objective synonym of *javensis* Horsfield.

4. All global and regional manuals, handbooks and checklists after Stresemann (1937) have consistently treated *cornutus* as the valid name for the Sumatran population (Peters, 1940; Howard & Moore, 1980, 1984, 1991; van Marle & Voous, 1988; Clements, 1991, 2000, 2007; Mackinnon & Phillips, 1993; Cleere, 1998; Holyoak, 1999, 2001; Dickinson, 2003; Clements, 2007). Most, but not all, the recent

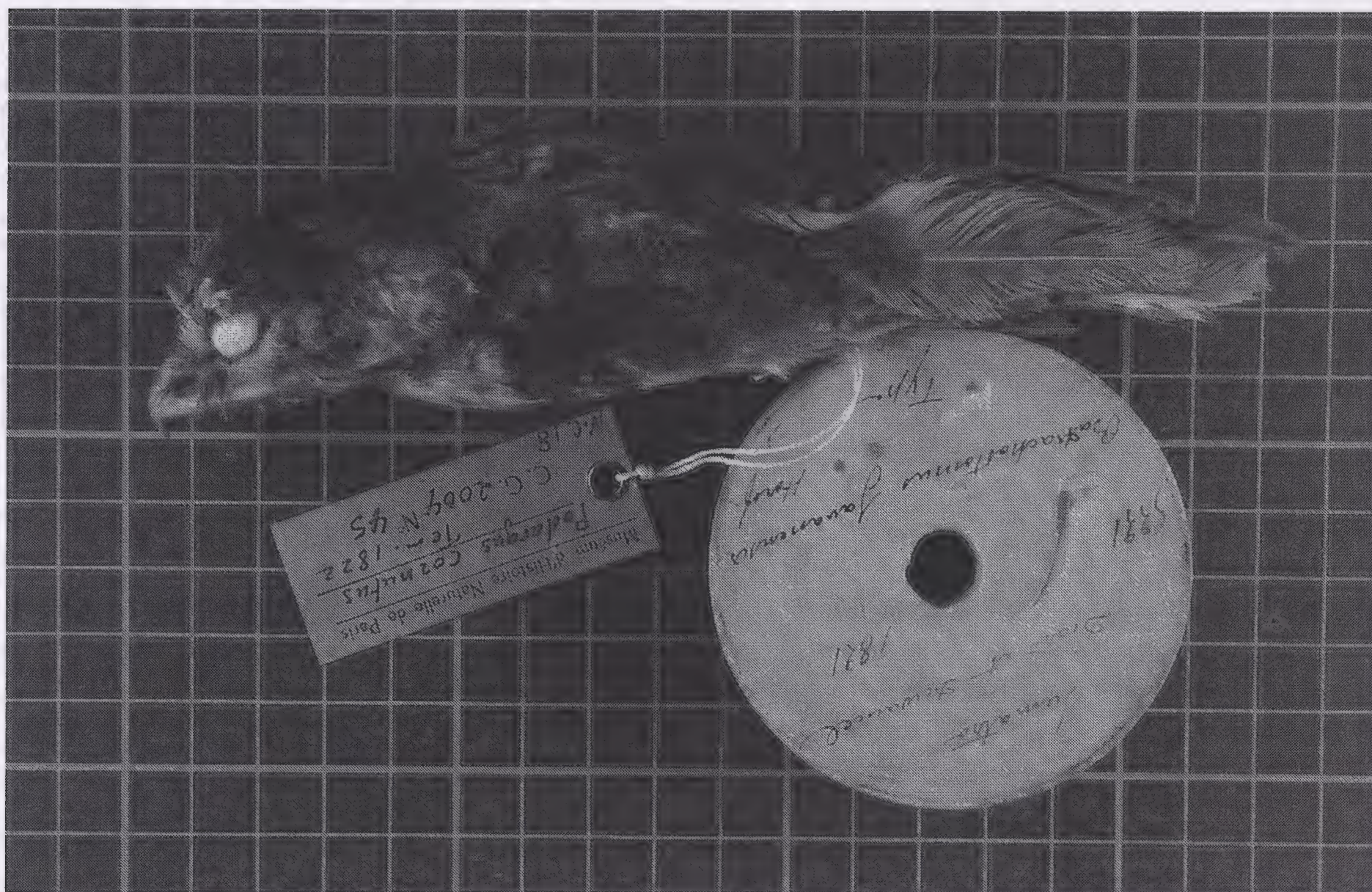


Fig. 1. The listed 'holotype' of *Podargus cornutus* Temminck, 1822, which we propose be accepted as the neotype. The squares that underlie the specimen are 1 cm by 1 cm.

authorities consider it a separate species and additionally associate with it the allopatric populations on Borneo and Bangka. The Commission has received a list of 10 additional publications further supporting the statement that the name *cornutus* has been universally used for this taxon since it was separated in 1937.

5. No other name has been traced for the distinct Sumatran form of this frogmouth; nor for the Bornean population. It has twice previously been suggested, however, that an application should be made to conserve *cornutus* for it (Inskipp et al., 1996; Holyoak, 2001). This requires the removal of *cornutus* from its position in junior objective synonymy with *javensis*. Under Article 75.6, we now ask to do this through the designation of a neotype from the Sumatran population based on a unsexed individual at the Muséum National d'Histoire Naturelle, Paris (MNHN), C.G. 2004–45 (A.C. Ancien Catalog: 5221) collected in Sumatra by Diard and Duvaucel (Fig. 1). This specimen is considered to be the one that Temminck (1822) claimed was the model for his Plate 159 (Cleere et al., 2006).

6. The International Commission on Zoological Nomenclature is accordingly asked:

(1) to use its plenary power:

(a) to rule that the name *Podargus cornutus* was proposed by Temminck (1822) for a new taxon, rather than as a replacement name for *Podargus javensis* Horsfield, 1821, thereby removing this taxon from junior objective synonymy with *Podargus javensis* Horsfield, 1821;

(b) to designate specimen C.G. 2004–45 (A.C. Ancien Catalog: 5221) in Muséum National d'Histoire Naturelle, Paris (MNHN), as neotype of *Podargus cornutus* Temminck, 1822, as deemed available in (1) above;

- (2) to place on the Official List of Specific Names in Zoology the name *cornutus* Temminck, 1822, as published in the binomen *Podargus cornutus*, as deemed available in (1) above and as defined by the neotype in the Muséum National d'Histoire Naturelle, Paris (MNHN), C.G. 2004–45 (A.C. Ancien Catalog: 5221).

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Case 3499***Anthochaera* Vigors & Horsfield, 1827 and *Philesturnus* Geoffroy Saint-Hilaire, 1832 (Aves): proposed conservation by suppression of the generic name *Creadion* Vieillot, 1816**

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Abstract. The purpose of this application, under Article 23.9.3 of the Code, is to conserve the generic names *Anthochaera* Vigors & Horsfield, 1827 and *Philesturnus* Geoffroy Saint-Hilaire, 1832 for Australian wattlebirds (MELIPHAGIDAE) and the New Zealand saddleback (CALLAEIDAE) by suppression of the generic name *Creadion* Vieillot, 1816. Suppression of the name *Creadion* is sought because, although used for the New Zealand saddleback up to the mid 20th century, it may instead be applied to the Australian wattlebirds, long known as *Anthochaera* Vigors & Horsfield, 1827. *Creadion* is therefore a source of confusion and threatens stability.

Keywords. Nomenclature; taxonomy; CALLAEIDAE; MELIPHAGIDAE; *Creadion*; *Anthochaera*; *Philesturnus*; saddleback; wattlebirds; Australasia.

1. Vieillot (1816, p. 84) introduced the genus *Creadion* for Australasian passerine birds with wattled bills. He diagnosed it briefly, and included in it three nominal species without designating any as type. They were '*Sturnus carunculatus*', '*Merops carunculatus*' and '*Corvus paradoxus*, Lath.' (= Latham). *Sturnus carunculatus* is the New Zealand saddleback, currently known as *Philesturnus carunculatus* (Gmelin, 1789) in the family CALLAEIDAE. *Merops carunculatus* and *Corvus paradoxus* Latham are the Australian mainland red and Tasmanian yellow wattlebirds, respectively known as *Anthochaera carunculata* (Shaw, 1790) and *A. paradoxus* (Daudin, 1800) in the family MELIPHAGIDAE. Vieillot's attribution of *Corvus paradoxus* to Latham was an error, as Latham, in his only use of the name (1801, p. xxvi), correctly quoted Daudin as its author.

2. Vieillot (1817, pp. 390–391) then placed *Sturnus carunculatus* in one unnamed section of *Creadion*, and *Merops carunculatus* and *Corvus paradoxus* together in another; later he had the last species figured alone as an example of the genus, under

the misapplied species name 'le Créadion à pendeloques, *Creadion carunculatus*' (Vieillot & Oudart, 1823, pl. 94). No steps were taken, however, to designate a type species as required by Article 67.5 of the Code. Next to become embroiled in the issue were Vigors & Horsfield (1827) in their landmark review of Australian birds in the collections of the Linnean Society of London. They referred both *Merops carunculatus* and *Corvus paradoxus*, together with other Australian meliphagids, to their new genus *Anthochaera* (Vigors & Horsfield, 1827, pp. 320–321). *Anthochaera* was described there in detail, with the summary comment (p. 321): 'M. Vieillot has referred (these birds) to a new genus of his, which he names *Creadion*, and which he divides into two sections; one represented by the *Sturnus carunculatus*, Gmel., the other by the bird(s) before us. He places this genus in the vicinity of the STURNIDAE. . . . As we consider our present group to have no relation to the STURNIDAE beyond . . . carunculated appendages to the head, we have no hesitation in removing it from the genus *Creadion*; leaving the *Sturnus carunculatus* (with which species . . . we are unacquainted) to represent that group'.

3. This statement was accepted by Amadon (1962, p. 158) as a subsequent designation of *Sturnus carunculatus* Gmelin (New Zealand saddleback) as the type species of *Creadion*. Yet Vigors & Horsfield's (1827, p. 321) use of the verb 'represent' does not appear to qualify as an equivalent designatory term, as required by Article 69.1.1 of the Code. Here we have taken our cue from Article 67.5 of the Code, which requires that the fixation of type species 'be rigidly construed'. In their discussion of *Anthochaera* and *Creadion*, Vigors & Horsfield (1827) were concerned with species relationships, not about fixing genera nomenclaturally by type species designation. Thus their use of the word 'represent' when referring *Sturnus carunculatus* to *Creadion* is in the context of a taxonomic example. As such, it would not qualify as a type species designation under Article 67.5.1 of the Code. Instead, Vigors & Horsfield's action appears to constitute 'fixation by elimination' which is also excluded as a type species designation by Article 69.4 of the Code. Neither Amadon (1962) nor Wolters (1980, p. 448) were aware of the ruling on 'fixation by elimination' because it did not come into effect until 1985, in the third edition of the Code (1985, Article 69 (b)).

4. The next, and clearly valid, type species designation for *Creadion* was not of *Sturnus carunculatus* Gmelin, but of a different originally included nominal species. Lesson (1837, p. 7) said of *Creadion*: 'Le type de ce genre, et sans contredit la seule espèce qu'on doive y admettre, est le créadion à pendeloques³, qui habite la terre de Diemen. . .' ['The type of this genus, and without contradiction the only species that we must admit to it, is the *Creadion* with pendeloques, which inhabits van Dieman's Land. . .']. The superscript 3 refers to a footnote which lists the following synonyms, in order: 'Pie à pendeloques, Daudin, t. 11, pl. 16; *creadion carunculatus*, Vieill., Gal., pl. 94; *merops carunculatus*, Lath.; *corvus paradoxus*, ib.; Shaw, White, p. 144 et 240; *anthochaera carunculata*, Horsf. & Vig.'. Although the synonymy is partly confounded by references to other species, the type species explicitly and implicitly named in Lesson's text is 'Le Créadion à pendeloques, *Creadion carunculatus*' of Vieillot (1823, p. 142, pl. 94 – see paragraph 2 above), together with its senior synonym, the Tasmanian 'Pie à pendeloques, *Corvus paradoxus*' of Daudin (1800, p. 246, pl. 16). This senior available binomen was cited in Lesson's synonymy. *Creadion* Vieillot, 1816 thereby becomes applicable to the Australian wattlebirds and senior to *Anthochaera* Vigors & Horsfield, 1827.

5. Drawing attention to Lesson's action, Mathews (1922, pp. 169–170) was the first to conclude that the type species of *Creadion* Vieillot was *Corvus paradoxus* Daudin. He then (Mathews 1925a, pp. 75–76) went on to determine that *Philesturnus* Geoffroy Saint-Hilaire, 1832, was the earliest – and valid – generic name for the New Zealand saddleback. Its type species is *Sturnus carunculatus* Gmelin, 1789 by monotypy. Geoffroy Saint-Hilaire (1832, p. 390) had misassigned its authorship to Latham, but this is corrected here under Article 67.7 of the Code. The actual author is J.F. Gmelin (1789, p. 805): Latham had correctly attributed the name to Gmelin in his only use of it (Latham, 1790, p. 324).

6. 19th and early 20th century ornithology, nevertheless, followed Vigors & Horsfield's generic nomenclature. From Gould (1848, 1865), Buller (1873, 1882), Sharpe (1877) and Gadow (1884) into the first two decades of the 20th century, *Creadion* Vieillot was employed for the New Zealand saddleback and *Anthochaera* (or *Acanthochaera* [sic.]) Vigors & Horsfield for the Australian wattlebirds. After that, Mathews' (1922, 1925a) findings generated a divergence in usage. In New Zealand, *Philesturnus* Geoffroy Saint-Hilaire was taken up for the saddleback by both the *Checklist of New Zealand Birds* (OSNZ Checklist Committee, 1953) and Oliver (1955), and it has been employed there for this endangered New Zealand endemic ever since. For over 50 years now, *Philesturnus* has been used in all official New Zealand checklists (OSNZ Checklist Committee, 1970, 1990), handbooks (Westerkov, 1967; Robertson, 1985; Peter, 2006), atlases (Bull et al., 1985; Robertson et al., 2007), field guides (Falla et al., 1966, 1979; Heather & Robertson, 1997; Robertson & Heather, 1999), and manuals of extinct and threatened birds (Gill, 1991). Moreover, there have been over 80 separate uses of *Philesturnus* for the saddleback compared to 9 of *Creadion* in the primary ornithological journal of New Zealand, *Notornis*, since its commencement in 1943. All uses of *Creadion* in that journal are earlier than 1960 except for one in 1977 and another in 1988.

7. In Australia, by contrast, Mathews' findings affecting *Anthochaera* were ignored. To avoid continuous splitting and nomenclatural alterations, *The Official Checklist of the Birds of Australia*, 2nd edition (RAOU Checklist Committee, 1926) and all subsequent Australian checklists (Schodde, 1975; Christidis & Boles, 1994, 2008; Schodde & Mason, 1999) kept *Anthochaera* Vigors & Horsfield for the Australian wattlebirds. Since the turn of the 20th century, the uses of *Anthochaera* for the Australian wattlebirds have run into the thousands. Two of its species are common urban birds in the capital cities of Sydney, Melbourne, Canberra and Adelaide; two are of economic concern as pest species in orchards in southern Australia and Tasmania; and another, *Anthochaera phrygia* (Shaw, 1794), is gazetted by national and State governments as endangered. The name *Anthochaera* is used for these species throughout Australia today, in handbooks, atlases, field guides and conservation manuals. In the senior Australian ornithological journal, *The Emu*, the name has been used in over 500 separate papers between 1900 and 2000 alone. In comparison, *Creadion* Vieillot was employed for the Australian wattlebirds only by Mathews, and in just five of his later works for only one of the species (Mathews, 1924, 1925b, 1930, 1931, 1946).

8. Outside Australasia, *Anthochaera* Vigors & Horsfield has remained in exclusive use for the Australian wattlebirds up to the present. It is employed for them in all

global manuals (Higgins et al., 2008) and checklists (Salomonsen, 1967; Gruson, 1976; Wolters, 1979; Howard & Moore, 1980, 1991; Sibley & Monroe, 1990, Monroe & Sibley, 1993; Wells, 1998; Dickinson, 2003; Clements, 2007). Outside New Zealand, *Creadion* Vieillot was reserved for the New Zealand saddleback for a much longer time, as in Gruson (1976), Howard & Moore (1980, 1991) and Sibley & Monroe (1990). Such usage followed 19th century tradition and Amadon's (1962, p. 158) interpretation in the respected and influential *Check-list of Birds of the World* begun by J.L. Peters at Harvard – see paragraph 3 above. Yet there has been more than a little confusion, exemplified by Wolters' world checklist which used *Philesturnus* Geoffroy Saint-Hilaire in the main text of that work (Wolters, 1977, p. 219) and then, without explanation, changed to *Creadion* Vieillot in an appendix of corrections (Wolters, 1980, p. 448). Since 1990, nevertheless, virtually all global manuals and checklists have, similar to those in New Zealand, turned to *Philesturnus* Geoffroy Saint-Hilaire for the saddleback (Monroe & Sibley, 1993; Wells, 1998; Dickinson, 2003; Clements, 2007).

9. As usage stands today, *Anthochaera* Vigors & Horsfield remains in universal use for the Australian wattlebirds, *Philesturnus* Geoffroy Saint-Hilaire has come into prevailing use globally as well as provincially for the New Zealand saddleback, and *Creadion* Vieillot is falling into disuse. Because of the differing interpretations to which the type species of *Creadion* has been and may be subject, that name has become a source of confusion (Bock 1994, p. 220). It has potential to generate serious ongoing instability, particularly if transferred from the New Zealand saddleback (CALLAEIDAE) to the Australian wattlebirds (MELIPHAGIDAE). The Standing Committee on Ornithological Nomenclature of the International Ornithological Committee reviewed the matter in 2002 and concluded unanimously that the simplest and most effective solution in the interest of stability was suppression of *Creadion*.

10. In this event, for *Anthochaera* Vigors & Horsfield to be entered on the Official List of Generic Names in Zoology, its type species needs to be clarified. Contrary to references in Gadow (1884, p. 262), the RAOU Checklist Committee (1926, p. 103) and Salomonsen (1967, p. 444), the first to designate one of the originally included nominal species as type species was G.R. Gray (1840, p. 15). He designated '*A. carunculata* (Lath.) V. & H.', which, in its original combination, is *Merops carunculatus* Latham, 1790. Latham's name was used as valid in the original description of *Anthochaera* (Vigors & Horsfield, 1827, p. 321), where, however, Vigors & Horsfield confounded its identity. Latham (1790, p. 276) had based *Merops carunculatus* exclusively on the 'Wattled Bee-eater' from Port Jackson, New South Wales, in Phillip's (1789) '*The Voyage of Governor Phillip to Botany Bay*', p. 164. Vigors & Horsfield (1827, footnote) nevertheless confined their own circumscription of *Merops carunculatus* Latham to material of *Corvus paradoxus* Daudin collected in Tasmania. Thus *Merops carunculatus* Latham in Vigors & Horsfield (1827, p. 321) is a misidentified nominal species. Gray (1840) expressly used Vigors & Horsfield's circumscription of *Merops carunculatus* in his designation of the type species of *Anthochaera*, as is evident from the full form of its specification: '*A. carunculatus* (Lath.) V. & H. Vieill. Gal. des Ois. Pl. 94'. '(Lath.) V. & H.' quotes Vigors & Horsfield's application of Latham's name, and 'Vieill. Gal. des Ois. Pl. 94' refers to plate 94 in *La Galerie des Oiseaux* (Vieillot & Oudart, 1823), which is also of the Tasmanian wattlebird – see paragraph 2 above. Gray's designation is thus a

deliberate use of a name in the sense of a previous misidentification as governed by Article 11.10 of the Code. Under Article 69.2.4 of the Code, the type species of *Anthochaera* then becomes *Corvus paradoxus* Daudin, 1800. It is the senior available and in-use epithet for the Tasmanian wattlebird, and also a nominal species included in the synonymy of *Merops carunculatus* Latham, 1790 in the original description of *Anthochaera* (Vigors & Horsfield, 1827). The designation of *Corvus paradoxus* Daudin as the type species of *Anthochaera* maintains the customary usage of this generic name in the family MELIPHAGIDAE.

11. The International Commission on Zoological Nomenclature is accordingly asked:

- (1) to use its plenary power to suppress the generic name *Creadion* Vieillot, 1816 for the purposes of the Principle of Priority but not for those of the Principle of Homonymy;
- (2) to place on the Official List of Generic Names in Zoology the following names:
 - (a) *Anthochaera* Vigors & Horsfield, 1827 (gender: feminine), type species by subsequent designation by G.R. Gray (1840) *Corvus paradoxus* Daudin, 1800;
 - (b) *Philesturnus* Geoffroy Saint-Hilaire, 1832 (gender: masculine), type species by monotypy *Sturnus carunculatus* Gmelin, 1789;
- (3) to place on the Official List of Specific Names in Zoology the following names:
 - (a) *paradoxus* Daudin, 1800, as published in the binomen *Corvus paradoxus* (specific name of the type species of *Anthochaera* Vigors & Horsfield, 1827);
 - (b) *carunculatus* Gmelin, 1789, as published in the binomen *Sturnus carunculatus* (specific name of the type species of *Philesturnus* Geoffroy Saint-Hilaire, 1832);
- (4) to place on the Official Index of Rejected and Invalid Generic Names in Zoology the name *Creadion* Vieillot, 1816, as suppressed in (1) above.

Acknowledgements

This application has been evaluated and approved by the Standing Committee on Ornithological Nomenclature (SCON) of the International Ornithological Committee. We are also particularly grateful to Dr W.D.L. Ride for his advice in interpreting Vigors & Horsfield's fixation of the type species of *Creadion* as a case of elimination.

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Comments on this case are invited for publication (subject to editing) in the *Bulletin*; they should be sent to the Executive Secretary, I.C.Z.N., c/o Natural History Museum, Cromwell Road, London SW7 5BD, U.K. (e-mail: iczn@nhm.ac.uk).

Comment on *Palaemon rosenbergii* De Man, 1879 (currently *Macrobrachium rosenbergii*; Crustacea, Decapoda): proposed conservation of usage by designation of a neotype

(Case 3428; see BNZ 65: 288–292)

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We have worked with *Macrobrachium rosenbergii* (De Man, 1879) for 35 years in Puerto Rico. EHW began working with a commercial shrimp farm in Cabo Rojo in 1975 and afterwards with other farms (Williams & Bunkley-Williams, 1999a, b; Williams & Sindermann, 1992; Williams et al., 2001). DEA began his research and that of his students on this animal at the Lajas Experiment Station in 1980 (e.g. Alston, 1989, 1991; Alston & Garcia-Pérez, 2000; Garcia-Pérez & Alston, 2000; Garcia-Pérez et al., 2000; Cole et al., in press) and cooperated on disease work (Alston & Bunkley-Williams, manuscript).

This shrimp was partially selected for aquaculture because of its lack of aggression (Alston, 1991). Despite frequent escapes in Puerto Rico, it has never become established. It has only been accidentally introduced in a couple of Caribbean localities where the native shrimp have been virtually eliminated (Williams et al., 2001). Despite product recognition and a high market value for this product in Puerto Rico, commercial aquaculture has had mixed results. It seems best suited for inland areas where it does not compete with marine shrimp or in polyculture with such fish as tilapia.

AFS and FAO have attempted to establish an approved common name (vernacular name) for this animal (e.g. FAO, 2009; McLaughlin et al., 2005; Williams et al., 1989) but have been virtually ignored in the cacophony of divergent, and often inappropriate, common names (e.g. freshwater prawn, freshwater Malaysian prawn, freshwater shrimp, Freshwater Shrimp, Fresh water[sic] Shrimp, giant freshwater prawn, giant fresh water[sic] prawn, giant freshwater shrimp, giant Malaysian prawn, giant Maleysian[sic] prawn, giant prawn, giant river prawn, Malaysian freshwater prawn, Malaysian prawn, prawn, scampi). The lack of any consistently used common name makes the stability of the scientific name all the more important.

We support the petition of Wowor & Ng (2007, 2008) to conserve the scientific name *Macrobrachium rosenbergii* (De Man, 1879) for this well-known and commercially important animal. Loss or re-application of this name would create confusion for aquaculture, conservation organisations, regulatory agencies, and the general public. This name has been very widely used in a myriad of publications in the sense suggested by Case 3428 for many decades without opposition or conflicting or alternative names.

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Comments on the proposed conservation of *Termes serratus* Froggatt, 1898 and *Termes serrula* Desneux, 1904 (Insecta, Isoptera, TERMITINAE)

(Case 3385; see BZN 64: 83–86, 185–187, 65: 47–49, 132–136)

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I have read Jones' (2007, 2008) (BZN 64: 83–86; 65: 47–49) proposal to conserve the junior specific names *Microcerotermes serratus* (Froggatt, 1898) for an Australian species and *Microcerotermes serrula* (Desneux, 1904) for a southeast Asian species, and Roisin & Pasteels' (2000; 2007 BZN 64: 185–157) and Roisin's (2008) argument to use the senior specific name *Microcerotermes serratus* (Haviland, 1898) for the Southeast Asian species and use the synonym *Microcerotermes parviceps* Mjöberg, 1920 for the Australian species. I wish to offer some views as an Australian-based researcher of termite ecology and management of pest termites in Australia.

I include in this comment 39 publications not previously cited by Roisin & Pasteels (2000, 2007), Jones (2007, 2008) or Kirton (2008). There are 22 publications using the name *Microcerotermes serratus* (Froggatt): Froggatt, 1915; Hill, 1921; Noirot, 1956; Gay, 1961; Anonymous, 1972–1973; Braithwaite, 1985; Hadlington, 1987; Braithwaite et al., 1988; Spain & Reddell, 1996; Andersen et al., 1998; Myles, 1999; Hinz, 2001; Lavelle & Spain, 2001; Dawes-Gromadzki, 2003, 2008a, 2008b; Dawes-Gromadzki & Spain, 2003; Gerozisis & Hadlington, 2004; Ahmed et al., 2005; Rasib, 2008; Staunton et al., 2008; Woodman et al., 2008; 17 publications using the name *Microcerotermes serrula* (Desneux, 1904): Kemner, 1934; Tho, 1982; Chey, 1997; Eggleton et al., 1997; Jeeva, 1998; Homathevi, 1999; Jeeva et al., 1999; Gillison, 2000; Gillison et al., 2003; Wolseley et al., 2001; Gathorne-Hardy et al., 2001, 2006; Homathevi et al., 2002; Rahman & Tawatao, 2003; Donovan et al., 2007; Lee, 2007; Anonymous, 2008; and one publication that uses both (Hegh, 1922).

The senior name *Microcerotermes serratus* (Haviland) has been used only once since the original description and the synonym *Microcerotermes parviceps* Mjöberg, 1920 was used once before Hill (1942) synonymised it with *serratus* (excluding Roisin & Pasteels, 2000). In contrast, of the 77 publications used by Roisin & Pasteels (2000, 2007), Jones (2007, 2008), Kirton (2008) and in this comment, the name *Microcerotermes serratus* (Froggatt) was used as valid in 46 publications and the name *Microcerotermes serrula* (Desneux) was used as valid in 31 publications. These numbers do not include the original descriptions (Haviland, 1898; Froggatt, 1898; Desneux, 1904).

The junior names have been used continuously over the past century: the table below shows the chronological distribution of all names from all 80 publications in Roisin & Pasteels (2000, 2007), Jones (2007, 2008), Kirton (2008) and in this comment, sorted to decade. A = description of *serratus* Froggatt, B = description of *serratus* Haviland, C = change of *serratus* to *serrula* for Malaysia species, D = first description of *parviceps* Mjöberg, E = Hill (1942) synonymised *parviceps* under *serratus* Froggatt, F = Roisin & Pasteels (2000) synonymised *parviceps* under *serratus* Haviland.

Decade	<i>serratus</i> Froggatt Australian	<i>parviceps</i> Mjöberg Australian	<i>serratus</i> Haviland Malaysian	<i>serrula</i> Desneux Malaysian
1890–1899	A		B	
1900–1909	1+C		C	C
1910–1919	2		1	1
1920–1929	4	1+D		1
1930–1939				1
1940–1949	1	E		
1950–1959	4			1
1960–1969	1		1	
1970–1979	3			
1980–1989	8			5
1990–1999	10			7
2000–2009	11+F	F	F	15+F

Article 23.9.11 has not been met because the senior name has been used as valid after 1899.

The conditions of Article 23.9.1.2 have been met because the junior name *M. serratus* has been used in 34 publications and *M. serrula* has been used in 27 publications in the last 50 years, both spanning periods greater than 10 years.

Roisin & Pasteels (2007) argued that their change would not cause substantial confusion for two reasons. The first reason was a lack of study on these species. However, both species have been recorded many times as part of biological surveys in natural forest and human modified lands (Ferrar & Watson, 1970; Holt & Coventry, 1982; Collins, 1984; Braithwaite, 1985; Braithwaite et al., 1988; Nkunika, 1988; Holt et al., 1993; Eggleton et al., 1997, 1999; Andersen et al., 1998; Hinz, 2001; Wolseley et al., 2001; Homathevi, 2002; Jones et al., 2002, 2003; Dawes-Gromadzki, 2003; 2008a, 2008b; Dawes-Gromadzki & Spain, 2003; Gathorne-Hardy et al., 2001, 2006; Gathorne-Hardy, 2004; Donovan et al., 2007). These 23 studies represent a substantial amount of biological study of these species.

The second reason was that these two species were not well known. Yet three lines of evidence show that they are well known. (1) A combined total of 76 publications use these names as valid. (2) The names are used in multiple government reports (Anonymous, 1972–1973; Braithwaite, 1985; Andersen et al., 1998; Gillison, 2000; Hinz, 2001; Anonymous, 2008; Woodman et al., 2008). (3) Both *M. serratus* and *M. serrula* are recognised as pest species by Australian and Malaysian Federal and State governments; ten official websites are listed below. The Australia Standard for pest termite management has included the name *Microcerotermes serratus* in various publications from 1966 to 1993. All pest control officers in Australia are familiar with the Standard for their industry, and several publications aimed at educating and/or regulating the pest control industry in Australia include the name *Microcerotermes serratus* (Froggatt, 1915; Hill, 1921; Gay, 1961; Hadlington, 1987, 1996; Verkerk, 1990; Gerozisis & Hadlington, 2004; Stauton et al., 2006); note two private company websites listed below. The situation in Malaysia is not dissimilar as pest control publications include the name *Microcerotermes serrula* (Rahman & Tawatao, 2003; Lee, 2007).

I therefore support the application by Jones to the Commission to conserve the specific names *Microcerotermes serratus* (Froggatt) and *Microcerotermes serrula* (Desneux). These names are in widespread and entrenched usage in Australia and Malaysia. A strict application of the Principle of Priority as advocated by Roisin & Pasteels (2000, 2007) would cause considerable confusion, scientifically, in industry and government.

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Websites

The name *Microcerotermes serratus* is found on eight Australian Government websites:

Australian Government, Department of the Environment, Water, Heritage and the Arts;

Australian Biological Resources Study:

http://www.environment.gov.au/biodiversity/abrs/online-resources/fauna/afd/taxa/Microcerotermes_serratus/names

Australian Government, Department of Agricultural, Fisheries and Forestry, Pests and Diseases Image Library:

<http://www.padil.gov.au/browseSpecies.aspx?menu=s1&group=3&id=46&taxa=Isoptera>

Australian Government, National Land & Water Resources Audit:

<http://nlwra.gov.au/files/products/national-land-and-water-resources-audit/pn21446/pn21446.pdf>

Australian Government, National Health and Medical Research Council:

<http://www.nhmrc.gov.au/PUBLICATIONS/synopses/withdrawn/dp1.pdf>

Commonwealth Scientific and Industrial Research Organisation, Australian National Insect Collection:

http://anic.ento.csiro.au/database/specimen_map.aspx?BiotaID=24812

Government of the State of New South Wales, Department of Primary Industries, Agricultural Scientific Collections Unit:

<http://www.agric.nsw.gov.au/Hort/ascu/staff/taxalist.htm>

Government of the Northern Territory, Department of Natural Resources, Environment, The Arts and Sport:

<http://nt.gov.au/nreta/environment/assessment/register/ord/pdf/eisch8.pdf>

Australian Museum; Fauna Net:

<http://www.faunanet.gov.au/MNLDetail.cfm?Kingdom=Fauna&NameID=INISOPT069gns>

The name *Microcerotermes serrula* is found on two Malaysian Government websites:

Malaysia Government, Department of Wildlife and National Parks:

<http://www.wildlife.gov.my/>

Malaysia Government, Forest Research Institute:

<http://www.frim.gov.my/>

The name *Microcerotermes serratus* is found on two private pest control company websites:

Allgone Pest – Brisbane Queensland

<http://www.allgonpest.com.au/index.php?pgid=35>

Superway – Brisbane Queensland

http://superway.com.au/index.php?option=com_content&task=blogcategory&id=22&Itemid=38

The names *Microcerotermes serratus* and *M. serrula* are found on six international websites:

<http://www.cybertruffle.org.uk/cgi-bin/nome.pl?organism=94644&glo=eng>

<http://www.biolib.cz/cz/taxontermlists/id579552/>

http://zipcodezoo.com/Animals/M/Microcerotermes_Genus.asp

<http://ctd.mdibl.org/detail.go?type=taxon&acc=139992>

<http://sib.uniprot.org/taxonomy/139992>

<http://ib.ist.hokudai.ac.jp/~tendo/etools/gene/inv.organism>

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I have carefully read Jones's proposal in conjunction with the arguments of Roisin & Pasteels (2000); BZN 64: 185–187 and Roisin (2008). I have also studied the argument put forward by Theodore A. Evans in support of Jones's case (this issue). My conclusion is that I wish to fully support the views of both Jones and Evans, by strongly recommending that the specific names *Microcerotermes serratus* (Froggatt) and *Microcerotermes serrula* (Desneux) be conserved.

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I am in complete agreement with the ideas proposed by T.A. Evans (this issue).

(4) Ross H. Crozier

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It is now clear that the names *M. parviceps* and *M. serratus* (Haviland) have been essentially forgotten since their descriptions until rediscovered by Roisin & Pasteels, whereas *M. serratus* (Froggatt) and *M. serrula* have been used in significant literature in the Asian and Australian regions.

It is worth remembering the main purpose of scientific nomenclature, namely to provide stable links between scientific studies so that researchers know the identities of species worked on by others in relation to their own studies. The other purpose is to accurately reflect similarities between organisms in higher classification systematics. The proposal by Roisin & Pasteels would do extreme damage to the first aim while doing nothing to assist the second, because no systematic changes are envisaged, only taxonomic ones.

It is bad enough when one name is substituted for another, but when a well-used name is transferred from one species to another, the resulting confusion is rather extreme. It is not enough to suggest that appending 'Haviland' to the name *M. serratus* would automatically make all clear. Firstly, most journals no longer require or even favour the addition of authors' names to species names. Secondly, most authors, if needing to meet this requirement, simply check the first paper in their reprint collection which gives the author's name. Authors' names are therefore a valueless crutch; attention has to be given to the best names to use and the Code is clear that stability is important, giving guidelines on how this can be achieved.

I therefore fully support continued use of the names *Microcerotermes serratus* (Froggatt) and *M. serrula*, and recommend that the name *M. serratus* (Haviland) be formally suppressed.

(5) Tracey Dawes

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I have recently been advised that there is the potential for the Commissioners of the International Commission on Zoological Nomenclature to vote against the conservation of the name *Microcerotermes serratus* (Froggatt, 1898). If this is the case, I believe that this will result in *M. serratus* becoming *M. parviceps* and the species *M. serrula* of Borneo now becoming *M. serratus*.

As an Australian senior research scientist specialising in termite ecology for over 10 years I have grave concerns if such an outcome was reached. I cannot support such a name change primarily due to the high degree of ongoing confusion and inconvenience that such a name change would bring about. This is primarily because (i) *M. serratus* is a well-known and recognised species of Australian termite and the change would assign this species a name that is unfamiliar, and (ii) this results in a relatively less well-known termite species from Borneo being suddenly assigned a name that has been associated with a well-known Australian termite for 100 years.

Additionally, for termite researchers wanting to classify termites, there is a relatively small but valuable set of reference works that are used. These include Hill (1942), Gay & Calaby (1970), Watson & Gay (1991) and Watson & Abbey (1993). In the event of the above name change, all these works would then carry the incorrect name for *M. serratus* and this would require significant effort, cost and time to rectify. The pest industry is also extensive across Australia and a name change would result in the entire industry carrying the incorrect name on all their pest literature. Also, all specimens in collections across the country and elsewhere would have to be relabelled.

From my knowledge and experience I do believe that in a broader context there will be significant and widespread negative consequences if such a name-change was permitted to occur. Such a change would lead to significant confusion and nomenclatural inconsistencies. Please consider this as a letter of support for the conservation of the name *Microcerotermes serratus* (Froggatt).

Additional references

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Comment on the proposed establishment of availability of *Balintus* d'Abrera, 2001, *Gulliveria* d'Abrera & Bálint, 2001, *Salazaria* d'Abrera & Bálint, 2001, *Megathecla* Robbins, 2002 and *Gullicaena* Bálint, 2002 (Insecta, Lepidoptera, LYCAENIDAE) (Case 3458; see BZN 65: 188–193, 66: 271–272)

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This is a reply to the comment by Craig (BZN 66: 271–272) on Case 3458.

This matter arose when d'Abrera & Bálint (2001) proposed eight generic names in the LYCAENIDAE (Lepidoptera). An application on the availability of these names (Robbins & Lamas 2008b) noted that the words in d'Abrera & Bálint (2001) differentiated the type species. Similarly, the characters in d'Abrera & Bálint (2001) differentiated the type species. The proposed interpretation (Robbins, 2002; 2004; Robbins & Lamas 2008a, b) was that d'Abrera & Bálint (2001) had differentiated the type species for each new generic name, contradicting Article 13.1.1 of the Code which requires differentiation of the new genus. Craig now proposes the alternative interpretation that d'Abrera & Bálint (2001) intended to differentiate each genus.

The first theme of our response to Craig is that promoting the stability and universality of names was the nomenclatural philosophy underlying all of our publications related to Case 3458 (Robbins, 2002; 2004; Robbins & Lamas, 2008a, b). The second theme of our response is that Craig's criticisms lack supporting reasons, explanations, and evidence.

Craig writes "The bone of contention revolves around a minor and rather trivial perceived technicality. Taking the example cited by the applicants, under the heading 'Genus *Annamaria* d'Abrera & Balint gen. nov.' the authors (in d'Abrera, 2001) wrote '... However, is distinguished from *Evenus* by ...' followed by several distinguishing characters of the wing veins and androchonial [sic] patches. This was clearly 'a definition that states in words characters that are purported to differentiate the taxon' in the sense of the Code (Article 13.1.1), in other words the authors' purpose in including this paragraph was to differentiate the new genus."

Our response is that Craig took the quote out of context. In the actual description, d'Abrera & Bálint (2001) wrote 'In NEOTROPICAL VII:1107 treated as *Evenus draudti*. Likewise by other workers. However, is distinguished from *Evenus* by ...' The subject of the first sentence is unequivocally the type species, not the genus. The subject of the second sentence is the type species. There is no indication that the subject of the third sentence is different. And in accord with this idea, the characters that d'Abrera & Bálint (2001) gave distinguish the type species, not the other species that they placed in *Annamaria*. Irrespective of whether this is a 'minor and rather trivial perceived technicality', Craig provides no evidence to support the notion that d'Abrera & Bálint (2001) intended to differentiate the genus.

Craig writes '*Chopiniana* [sic] is presumably equally sound in the absence of any comments to the contrary.' Is *Chopinia* d'Abrera (*Chopiniana* is a misspelling) nomenclaturally sound? The original description of *Chopinia* clearly listed characters that differentiate the male of the type species, *Thecla mazurka* Hewitson, from *Macusia* Kaye; none of these characters were noted to differentiate *Chopinia* from *Macusia*, in violation of Article 13.1.1. For this reason, we consider *Chopinia* to be unavailable (Robbins, 2004; Robbins & Lamas, 2008b). Craig continues 'This suggests that it is not the taxonomic concepts that the applicants find ambiguous or unacceptable, but rather the names themselves and/or their authorship.' This inappropriate statement is unsupported by fact.

Concerning the monotypic genera proposed by d'Abrera & Bálint (2001), Craig writes 'Although denied by the applicants, the characters of the type species are the characters of their respective monotypic genera; there are no other characters the authors could have provided to differentiate these taxa.' We are perplexed why Craig asserts that we denied that the characters of a monotypic genus are the same as those of the type species. Rather, we had originally accepted the availability of the monotypic genera because differentiating the type species of a monotypic genus could be interpreted as being equivalent to differentiating the genus (Robbins & Lamas, 2008b). However, in reviewing the original version of our application, an anonymous Commissioner responded 'As one of the authors of this potential Case is actually a Commissioner, I sent a mail to all Commissioners with the examples given above asking for advice on how exactly to interpret Article 13.1. The replies I got agree with my own interpretation that the monotypic genera are NOT made available through the description of the type species and only congener.' Our application agreed with this viewpoint because we knew of no compelling reasons to the contrary.

Craig writes 'Yet the Commission is being asked to reward this error of Robbins (2002) by making *Megathecla* available, while rejecting most of the new genera of d'Abrera & Bálint (in d'Abrera, 2001) as a punishment for their ambiguous phraseology.' We are perplexed how Craig can view a Commission decision as a reward or a punishment. The words 'reward' and 'punishment' do not occur in the Code. As we understand it, the Commission is charged with promoting the stability and universality of names, not with rewarding or punishing authors or applicants.

Craig writes 'The Commission should not consider rejecting some generic names that are inconvenient to the applicants while making others available.' Robbins & Lamas (2008b) provided evidence that their nomenclatural proposal was the most stable solution. We are perplexed by, and object to, Craig's assertion that our proposal was motivated by inconvenience, especially since he does not provide a single reason why these names would be 'inconvenient'.

Craig writes 'Such a ruling would engender ill-feeling and a sense of injustice and would exacerbate the nomenclatural confusion currently caused by this issue.' Although 'ill-feeling and a sense of injustice' are not specifically mentioned in the Code, a sense of fair and equal treatment is arguably an important foundation for all human endeavours. It is exactly the reason why the Code and its objectives apply to everyone fairly and equally. In this case, d'Abrera & Bálint (2001) unfortunately did not follow the rules, and Robbins & Lamas (2008b) addressed the question of how best to achieve the objectives of the Code. In what way can that be construed as unjust? How can it engender ill-feeling? Once again, Craig's assertions of ill-feeling and injustice are not accompanied by supporting evidence.

The reason for the application by Robbins & Lamas (2008b) was that a ruling by the Commission, one way or the other, would stabilise the currently confused nomenclature of the generic names proposed by d'Abrera & Bálint (2001). Craig does not explain his assertion that such a ruling would 'exacerbate' confusion, and quite frankly, we are perplexed by this statement.

Craig writes 'The Commission would be rubberstamping various breaches of good zoological practice (see the Code, Appendix A, Code of Ethics) that have already occurred in the course of this matter.' Craig does not specify the 'breaches', but we can think of several ethical issues related to 'this matter'. The only one, however, that has seriously hindered the promotion of the stability and universality of names, to which all provisions of the Code are subservient, was when d'Abrera & Bálint's nomenclatural actions did not meet the requirements of Article 13.1.1. This 'breach' resulted in an unstable nomenclature that is now taking the time of the Commission and the Secretariat.

Craig writes 'It would also set an unacceptable precedent that any taxonomist who might interpret the description of an earlier taxon as being inadequate, or as meaning something which conflicts with the Code, could correctly declare it unavailable and rename it her/himself.' We are truly perplexed with this sentence for two reasons. First, nomenclatural changes are made all the time; how can the changes proposed by Robbins & Lamas (2008b) be rationally called an 'unacceptable precedent'? Second, the objective of changing nomenclature is to stabilise names, as in the application of Robbins & Lamas (2008b), not to 'rename it her/himself'.

Robbins & Lamas (2008b) provided evidence that their nomenclatural solution was the most stable one with regard to usage in published papers and on websites. Craig proposes an alternative nomenclatural solution, but does not provide a single piece of evidence to support its stability with regard to current and past usage. Even if the solution that we proposed is not a good one (and we explained in our application why we think that it is the best one), our solution is consistent with the objectives of the Code. Alternatively, Craig's solution and his criticisms of our solution are, at best, inconsistent with the objectives of the Code.

(2) Marcelo Duarte

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I have been following the ongoing debate on Case 3458 (BZN 65: 188–193). I have also had the opportunity to read an unpublished reply (now BZN 66: 349–351) by Robbins & Lamas to Craig's comment (BZN 66: 271–272) on this case. I disagree with Craig's interpretations and consider them incorrectly supported according to the meaning of Article 13.1 of the Code. Robbins & Lamas (2008) were very meticulous and correct with their support, explanation, and reasons for proposing (1) the availability of five generic names; (2) four generic names to be placed on the Official List of Generic Names in Zoology, and four specific names to be placed on the Official List of Specific Names on Zoology; (3) eight generic names to be placed on the Official Index of Rejected and Invalid Generic Names in Zoology. The intention of the present comment is to support Case 3458 which was, in my opinion, properly intended to stabilise the usage of names in Neotropical butterflies.

Comment on the proposed conservation of the specific name of *Chrysophanus florus* Edwards, 1884 (currently *Lycaena florus*) (Insecta, Lepidoptera, LYCAENIDAE) by designation of a neotype for *Polyommatus castro* Reakirt, 1866 (currently *Lycaena castro*)

(Case 3450; see BZN 66: 136–143; 273)

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This comment is sent for the purpose of offering complete support for the conservation of the taxon *Lycaena florus* and the designation of a (new) neotype for the taxon occasionally known as *Lycaena castro*.

Dr James A. Scott has presented a very fine, detailed case for this position and I will not attempt to condense, repeat, or elaborate on his statements, but I will state that I completely and wholeheartedly support his position on this matter.

I am a very strong researcher into the butterfly genus *Lycaena* (Coppers). I have personally used the subspecific (or specific) name *florus* over the name *castro* for decades.

Comments on the proposed conservation of usage of *Testudo gigantea* Schweigger, 1812 (currently *Geochelone (Aldabrachelys) gigantea*; Reptilia, Testudines)

(Case 3463; see BZN 66: 34–50, 80–87, 169–186)

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One of the premises for considering the name *Testudo dussumieri* as appropriate for the Aldabra tortoise is that Gray's (1831, p. 9) brief diagnosis refers to a specimen in the Leiden Museum, allegedly collected by 'M. Dussumiere' on 'Insula Aldebra'. Hubrecht (1881, p. 44) reported a 'young specimen' in the spirit collections of this museum, stating: 'The locality from whence the specimen was brought is sharply fixed. Dussumier himself on his travels in the tropics collected it in the island of Aldabra . . .'. Recently, Bour (2006, p. 21) designated this specimen (RMNH 3231) as the lectotype of *T. dussumieri*. However, there is no evidence that Dussumier ever visited Aldabra, or that the lectotype is from this atoll.

Neither Dussumier himself, in his own published accounts of his collecting (e.g. correspondence in *Mémoires du Muséum d'Histoire Naturelle* of 1827, volume 15, pages 377–384), nor others who have reported on his travels (reviews by Arvy (1972) and Laissus (1973)) ever mention Aldabra as one of the localities visited by him. Dussumier was an obsessive if not compulsive fish collector, but none of his specimens (many referred to in Cuvier and Valenciennes's 1828–1848 *Histoire des Poissons*) are known to have been collected in the waters of Aldabra. A study of the labels of Dussumier's fish specimens in the Muséum National d'Histoire Naturelle (MNHN) in Paris found no mention of Aldabra as a collection locality (Philippe

Keith, MNHN, personal communication). Finally, the atoll is never mentioned in any of Dussumier's manuscript notebooks or papers in the archives of the MNHN in Paris (personal observation).

On the other hand, it is known that Dussumier called at the granitic islands of Seychelles, including Mahé. Mahé is mentioned specifically on the labels of some of his fish specimens in the MNHN, e.g. *Scomberoides tala* (Cuvier, 1832), *Etelis carbunculus* Cuvier, 1828, *Parupeneus rubescens* (Lacepède, 1801) and *Upeneus sulphureus* Cuvier, 1829. Moreover, he collected birds, reptiles and amphibians known to occur only in the granitic islands of Seychelles, and unknown from low lying, coralline outer islands like Aldabra Atoll, e.g. a chameleon, a snake and a tree frog mentioned in his letter published 1827 (referred to above), the dove *Streptopelia picturata rostrata* (Bonaparte, 1855) (label quoted by Voisin et al., 2005) and the sunbird *Nectarinia (Cinnyris) dussumieri* Hartlaub, 1860 (Oustalet (1878)). The young tortoise mentioned by Duméril & Bibron (1835, p. 114: 'Nous soupçonnons appartenir à la Tortue Éléphantine, une très jeune Chersite de treize centimètres de longueur, qui a été rapportée des îles Séchelles et donnée au Muséum d'histoire naturelle par M. Dussumier.') – apparently the same specimen that was later mentioned by Hubrecht (1881) – must also have been collected in the granitic islands of Seychelles, not Aldabra, one of the islands in the Mozambique Channel area that Duméril & Bibron (1835) listed under the distribution of *T. elephantina*. At the time of Dussumier's travels 'Seychelles' did not officially include Aldabra, which is more than 1000 km to the southwest of Mahé. Aldabra's administrative status was unclear well into the second half of the 19th century, with both the British (who had already taken over the granitic Seychelles) and the French still eyeing the atoll. It was only in 1879 that the British Board of Civil Commissioners petitioned for additional islands, including Aldabra, to form part of Seychelles, politically speaking. It was not until 1892 – nine years after Dussumier's death – that a British ship was sent to confirm formal possession of Aldabra and its inclusion in the dependency of Seychelles (McAteer, 2000, pp. 176–178).

Moreover, the manner in which Gray allegedly obtained at least some specimens collected by Dussumier creates doubt as to the correctness or completeness of the collecting data he received with the specimens. The mollusc expert Geoffrey Nevill, in a letter dated 5th March 1883 to the Azorean naturalist Francisco Arruda Furtado, discussing the slug *Mariaella dussumieri* Gray, 1855, (from a French collection purchased in London according to Gray) reported: '... there was a great row – the French party said Gray was tempting men to steal and sell him their specimens – all Dussumier's coll[ection] belonged to Jardin des Plantes. Gray retorted and said it was not his fault, if the officers of the Paris Museum sold their specimens on the sly ...' (Arruda, 2002). Discussing the same slug, Humbert (1862) pointedly described as 'singulier' (i.e. strange) that Dussumier's specimens from Mahé in the MNHN had made their way to London and been sold to the British Museum. *Mariaella dussumieri*, said by Gray (1855) to have been collected by Dussumier on 'Mahi near Sechelles', is now not included in the molluscan fauna of Seychelles by Gerlach (2006).

In any case, it would be very atypical of Dussumier, an 'infatigable collecteur' (Cuvier, 1831), to collect only a young tortoise and no other specimen from Aldabra, when one considers the wealth of animal life, birds especially, encountered later by (e.g.) Abbott (1894). To conclude, there is no evidence that Dussumier ever landed on

Aldabra and, therefore, no evidence that the Leiden Museum specimen designated by Bour (2006) as the lectotype of *T. dussumieri* was collected on Aldabra Atoll. Hence, adopting the actions petitioned in Case 3463 is the simplest, least disruptive way to bring lasting nomenclatural stability to the Aldabra tortoise, and comply fully with the spirit of the Code.

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In my opinion Frazier's proposal is completely unnecessary, because the facts are clear and the rules of the ICZN provide solutions for this situation. I support the arguments presented by Bour & Pritchard (BZN **66**: 169–174).

Both Bour (1984) and Pritchard (1986) came to the conclusion that the type specimen of *Testudo gigantea* used by Schweigger (1812) was not an Aldabra tortoise. Based on differences between Schweigger's description and Aldabra tortoises and the fact that the specimen came from Brazil, Pritchard (1986) reached the conclusion that Schweigger's type specimen was a large *Chelonoidis denticulata* (Linnaeus, 1766), a South American tortoise. At the time this hypothesis could not be checked because the type specimen seemed to be missing. Bour (2006) reported the rediscovery of the type specimen in the Paris museum collection, and indeed it turned out to be a *C. denticulata*. Bour's description and the pictures he published do not leave any doubt about its correct identification as *C. denticulata*. Thus, Pritchard (1986) turned out to be right. The minor differences in measurements given by Schweigger (1812)

and the present measurements in mm can easily be explained because of the different system used by Schweigger in 1812 and our metric system. Schweigger used feet, inches and lines, but he did not specify which of many options; he could have used the Paris foot because he was working in Paris, but as he was German he also could have used the Rheinlandish foot, and there are more possibilities, none of them providing the same metric measurements, but all well within the area of the published metric measurements. Thus, I think these minor differences can not be used to invalidate the rediscovery of the holotype of *Testudo gigantea*. I have no doubt that Bour's (2006) conclusions are correct. Frazier (2006), in order to stabilise nomenclature of the Aldabra tortoise (thus for purely nomenclatural reasons), designated a neotype of *Testudo gigantea*. This designation does not qualify under the conditions set out in Article 76.3.6, which states that a neotype should come as nearly as possible from the original type locality, in this case Brazil. Designating a neotype from Aldabra is a bit too far away from Brazil. The claim of Frazier (BZN 65: 34) that the neotype was designated to stabilise the nomenclature of the Aldabra tortoise is not very impressive, considering the different names that have been used in the past 25 years, as shown by Frazier (BZN 65: 34–43) himself. There was not much to stabilise. After the rediscovery of the holotype of *Testudo gigantea*, the designation of a neotype becomes void anyway (Article 75.8) and the specimen designated as neotype (USNM 269962) no longer has any name-bearing status. Bour (1984, 2006) pointed out the existence of a juvenile Aldabra tortoise in the collections of the Natural History Museum in Leiden, The Netherlands (RMNH 3231, from Aldabra, collected by J.-J. Dussumier) that was one of the two syntypes of *Testudo dussumieri* Gray, 1831, and Bour (2006) designated this specimen as the lectotype of that taxon.

Thus we are faced with the rather simple situation that a long lost holotype has been rediscovered without the shadow of a doubt about its identity. This holotype belongs to a different species from the Aldabra tortoise and its name and all other names attached to it, including generic names, fall into the synonymy of *Chelonoidis denticulata*. For the next available name there is a well-preserved, unequivocally identified lectotype, which should serve as name-bearing type for the Aldabra tortoises. The specific name *dussumieri* has already been in regular use since 1995, generally in the combination *Dipsochelys dussumieri* (Gray, 1831), a name that I think is correct and based on a lectotype without doubts regarding identity and type locality. This way the situation really would be stabilised, but in a different way from that proposed by Frazier.

Many of the comments in favour of Frazier's proposal emphasise the risk that a name-change would pose for the conservation of this taxon. I fail to see the problem, as there is no doubt about the identity of the taxon to be protected, i.e. the tortoise living on Aldabra Island. Statements that a different name would jeopardise this protection are exaggerated, as international bodies like CITES are able to change names of species on their lists with few problems and without jeopardising the protection of the taxa in question. And the same holds true for governments and their agencies. As for the argument that much of the older ecological literature uses the specific name *gigantea*, this is true, but that does not signify that the name could not be changed. It would be a rather simple equation to point out that '*gigantea*' has now become '*dussumieri*', independent of genus name. Due to changing taxonomic insights, the species has gone through a number of generic name changes that never attracted much attention and were accepted without much ado.

I recommend that the ICZN rejects the proposal of Frazier to use its plenary power (in my opinion a measure of last resort) to conserve the use of *Testudo gigantea* and suppress the name *Testudo dussumieri*. The articles of the Code clearly point out what to do in cases such as the present one, and no intervention of the Commission should be necessary.

(3) Jeremy Austin

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I am writing to you in strong support of Case 3463 to stabilise the name of the Aldabra tortoise. During my time as a research fellow at the Natural History Museum (London) I conducted phylogenetic and population genetic analyses of both living and museum material from the Seychelles, including Aldabra. At the time the nomenclatural instability was a major issue, for scientific understanding of the evolutionary history of these tortoises but more importantly for conservation of surviving populations. Since then the issues surrounding the name of the Aldabra tortoise appear to have become even more mired in a debate which ultimately cannot be resolved using historical museum collections alone. The recent proposal to establish an unambiguous neotype has great merit, from the perspective of stabilising the taxonomy of this very important species (it is the only surviving giant tortoise in the Indian Ocean) and to assist with conservation of the species. Continued debate about the name of the Aldabra tortoise will only serve to distract from and complicate these important conservation efforts.

Once again, I strongly support Case 3463 made by Dr Frazier and sincerely hope the ICZN will pass a ruling in favour of the proposal.

(4) Thomas Althaus

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In my function as acting Chair of the CITES Animals Committee (since 2002), I write these lines to you in regard to the issue of stabilising the name of the Giant Aldabra Tortoise as *Testudo gigantea*. I wish to support the opinion expressed by Dr Ute Grimm (BZN 66: 283) and confirm that this represents the position of the CITES Animals Committee.

For many years now it has been the policy of the Animals Committee and its nomenclature specialists to agree to and adopt taxonomic name changes of CITES-listed species only very reluctantly and very conservatively. The reason is that any change in nomenclature and in particular in CITES nomenclature leads to a highly complicated and expensive chain of follow-up events like adjusting each and every document, publication (including legislation) and computer file on trade data etc. in the 175 CITES Parties, not to mention the adjustment of every instruction material, identification manual and the information about this for all stakeholders (traders, scientific institutions, border control agents, government agencies etc.). The conservation gain for the species involved from such an exercise, which binds a lot of

resources, is practically zero or could have even a negative impact on the species concerned, as described by Dr Grimm in her comment. It is therefore no surprise that the Animals Committee supports that the names *Aldabrachelys* and *Testudo gigantea* for the Giant Aldabra Tortoise be conserved and *T. dussumieri* Gray, 1831 be suppressed.

Comment on the proposed conservation of usage of *Archaeopteryx lithographica* von Meyer, 1861 (Aves) by designation of a neotype
(Case 3390; see BZN 64: 182–184, 261–262; 65: 314–317; 66: 87–88)

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I disagree with the proposal by Bock & Bühler to designate a neotype for *Archaeopteryx lithographica* because there is no demonstrated need to do so. First, there is no reasonable disagreement that the name was applied to the feather, the first specimen discovered, by Hermann von Meyer (1861). There is a possibility that von Meyer later intended the name also to apply to the first discovered skeletal specimen, which is now in the Natural History Museum in London, although this is ambiguous and in any case irrelevant, because the referral of the skeleton was secondary. It has been noted that some authors have mistakenly regarded this skeletal specimen as the type, but that is not a reason to change it.

Secondly, there is no convincing evidence that the feather and the ten skeletal specimens do not belong to the same taxon. Although at one time or another most of these individual specimens have been given a different taxonomic name by various authors (Elzanowski, 2002), it has been on the basis of minor and inconsistent variations. No comprehensive anatomical and morphometric study, incorporating all possible hypotheses of populational, ontogenetic, sexual, and teratological variation, has shown convincingly that more than one taxon is represented in this material. Furthermore, no non-archaeopterygid theropod from the Solnhofen limestones is known to have possessed feathers, let alone feathers that correspond to the holotype. Nor has it been shown that the holotype feather is taxonomically distinct from those found in other specimens of *Archaeopteryx* from Solnhofen. Although it is true that the single feather provides few diagnostic characteristics, unless there is good reason to think that more than one taxon is represented among these specimens, there is nothing to correct.

Third, the presence of feathers in other fossil birds, for example those of the Jehol Biota in China, noted by some authors, is irrelevant to this case. The fossils of the Jehol Biota are some 20 million years younger than *Archaeopteryx*; they preserve a variety of types of feathers and proto-feathers, many of which are not found in *Archaeopteryx* or any later bird (Padian et al., 2001), and they represent a variety of theropod taxa not found in the Solnhofen limestones, none of which belong to ARCHAEOPTERYGIDAE.

Finally, if a neotype were to be designated, it should be based on the best available skeletal specimen, regardless of the order of discovery. In that case the London

specimen would not be the optimal choice, because although it preserves some very good and unusual features, such as the braincase, it is not as complete as the Berlin specimen, which is perhaps the most famous individual fossil in the world, judging from its ubiquitous representation in textbooks, advertisements, and other media. The Berlin specimen moreover is completely feathered, and its size suggests that it is adult or nearly so, in contrast to the Eichstatt specimen, which is also complete but lacks feathers and is generally regarded as a juvenile.

Names and type specimens should be conserved when there is no reason to think that the holotype and referred specimens, however incomplete, belong to different taxa. Until and unless a convincing case is made, on the basis of careful variational analyses of all available specimens, that one or more skeletal specimens cannot belong to the taxon represented by the holotype feather, they should all be referred to *Archaeopteryx lithographica*. In the event that one or more skeletal specimens must belong to a different taxon, then such specimens should be given a different name, as has been sometimes proposed by various authors (Elzanowski, 2002). For all these reasons the feather should remain the holotype until there is consensus that more than one species is represented in the fossil population at Solnhofen.

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Comment on the proposed conservation of usage of *Mastodon waringi* Holland, 1920 (currently *Haplomastodon waringi*; Mammalia, Proboscidea) by designation of a neotype

(Case 3480; see BZN 66: 164–167)

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The alfa-taxonomy of *Haplomastodon* is still controversial and I agree with Lucas (Case 3480) that the name of the only recognised South American species of this Pleistocene proboscidean genus should be stabilised.

Two names were recurrently used for this taxon: *Mastodon waringi* Holland, 1920 (now *Haplomastodon waringi*) and *Mastodon chimborazi* Proaño, 1922 (now *Haplomastodon chimborazi*, not *H. chimborazai* as erroneously reported by Lucas). Of the two, the usage of the name *H. waringi* is indeed far more widespread in the literature, after the systematic revision of Simpson & Paula Couto (1957). However, several authors questioned the validity of this species (e.g. Hoffstetter, 1955;

Ficcarelli et al., 1995, Ferretti, 2008; Ferretti, in press). As a matter of fact, Lucas himself, having revised the type material from Pedra Vermelha, Brazil, concluded that '*M. waringi* Holland, 1920 should be considered as a nomen dubium because it is based on undiagnostic material (Lucas, 2008; BZN 62: 164–167). I completely agree with this conclusion, and have expressed the same opinion in my revision of Ecuadorian *Haplomastodon* (Ferretti, in press). On the other hand, the species '*M. chimborazi* Proaño, 1922 is valid, as the original description and available published figures of the type skeleton from Punin, Ecuador clearly distinguish it from other similar brevirostrine gomphotheres. Despite this, the name *H. chimborazi* has received a minor consent among specialists, though it has continued to be used up to this day (Arauz, 1950; Costales Samaniego, 1950; Hoffstetter, 1950, 1952, 1955, 1986; Ficcarelli et al., 1995, 2003, Coltorti et al., 1998; Ferretti, in press). To note is that, contrary to common opinion, the type skeleton of '*M. chimborazi* was not completely lost in the 1929 fire of the Quito University. Hoffstetter (1952: 195) in fact identified, among the palaeontological collection of the Universidad Central de Ecuador, Quito, three skeletal elements, namely the atlas and two humeri, belonging to the original type specimens described by Proaño. The left and right humeri are currently kept at the Museo de Historia Natural of the Instituto de Ciencias Naturales (MICN) of the Universidad Central de Ecuador (UCE), Quito (catalog numbers MICN-UCE 1982 and 1981, respectively; Ferretti, in press). The atlas has not yet been located, though this specimen might well be in another collection of the UCE (Ferretti, in press). For these reasons, the proposal of Ficcarelli et al. (1995) to designate a neotype of the species is not valid according to the ICZN rules.

In conclusion, the evidence discussed above requires, in my opinion, that the name *warangi* be abandoned in favour of *chimborazi*. In so doing, priority is saved as it is recognised that '*M. waringi* is a nomen dubium and that '*M. chimborazi* is the oldest valid name with a diagnostic type.

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- Hoffstetter, R. 1986. High Andean mammalian faunas during the Plio-Pleistocene. Pp. 218–245 in Vuilleumier, F. & Monasterio, N. (Eds.), *High altitude tropical biogeography*. Oxford University Press, New York.

OPINION 2233 (Case 3417)***Malmgrenia* McIntosh, 1874 (Annelida, Polychaeta, POLYNOIDAE):
usage conserved**

Abstract. The Commission has ruled that the usage of the generic name *Malmgrenia* McIntosh, 1874 (a polynoid worm genus from the northern North Atlantic) is conserved by designation of *Malmgrenia andreapolis* McIntosh, 1874 as the type species of this genus.

Keywords. Nomenclature; taxonomy; Polychaeta; POLYNOIDAE; *Malmgrenia*; *Malmgrenia whiteavesii*; *Malmgrenia andreapolis*; scale worm; northern North Atlantic.

Ruling

- (1) Under the plenary power it is hereby ruled that all previous fixations of type species for the nominal genus *Malmgrenia* McIntosh, 1874 are set aside and *Malmgrenia andreapolis* McIntosh, 1874 is designated as the type species.
- (2) The name *Malmgrenia* McIntosh, 1874 (gender: feminine), type species by designation in (1) above *Malmgrenia andreapolis* McIntosh, 1874 is hereby placed on the Official List of Generic Names in Zoology.
- (3) The name *andreapolis* McIntosh, 1874, as published in the binomen *Malmgrenia andreapolis* (specific name of the type species of *Malmgrenia* McIntosh, 1874), is hereby placed on the Official List of Specific Names in Zoology.

History of Case 3417

An application to conserve the current usage of the widely used name *Malmgrenia* McIntosh, 1874 (a polynoid worm genus from the northern North Atlantic) by designation of *Malmgrenia andreapolis* McIntosh, 1874 as the type species of *Malmgrenia* was received from A.I. Muir (*Natural History Museum, London, U.K.*) and S.J. Chambers (*The National Museums of Scotland, Edinburgh, U.K.*) on 23 February 2007. After correspondence the case was published in BZN 65: 12–16 (March 2008). The title, abstract and keywords of the case were published on the Commission's website. No comments were received on this case.

Decision of the Commission

On 1 March 2009 the members of the Commission were invited to vote on the proposals published in BZN 65: 14. At the close of the voting period on 1 June 2009 the votes were as follows:

Affirmative votes – 17: Alonso-Zarazaga, Bogutskaya, Brothers, Fautin, Halliday, Kottelat, Krell, Lamas, Lim, Minelli, Pape, Papp, Patterson, Rosenberg, Štys, van Tol and Zhang.

Negative votes – 3: Grygier, Kullander and Ng.

Bouchet abstained. Pyle was on leave of absence.

Štys, voting FOR, said that the case concerns one genus name, *Malmgrenia* McIntosh 1874, typified by monotypy by *Malmgrenia whiteavesii* McIntosh, 1874

(nomen dubium) from Canada (syntypes lack diagnostic characters and are apparently lost). Two British species are also included in the genus, i.e. *M. andreapolis* McIntosh, 1874 and *M. castanea* McIntosh, 1876 (syntypes preserved in the NHM, London). The authors of the application propose *M. andreapolis* as a type species, and a lectotype of this species should be selected from syntypes at this opportunity or the reason why this is not done should be provided.

Grygier, voting AGAINST, stated that the best solution for this problem would be the eventual designation of a neotype from the Gulf of St. Lawrence for *M. whiteavesii*. The authors should have checked whether any specimens satisfactory for this purpose exist in Canadian collections. Voting AGAINST, Kullander also said that this case could be resolved with an appropriate neotype for *M. whiteavesii*. Also voting AGAINST, Ng said that it does make sense to change the type species and keep the status quo. However, here he voted against only because he was concerned that in lieu of a revision of the genera and species considered, one may need in the future to perhaps establish a new genus for the Canadian species. He would have thus preferred for the applicants to ask the Commission to declare the type material unusable and to be replaced by a neotype – from Canada if possible. He said he did not sense that it was known whether the Canadian and English species are conspecific. If this species is common in England, he thought it should also be common off Canada. This would fix the name and the genus once and for all. The name by McIntosh for the Canadian species is available as rules stand; it is just that the type is useless.

Bouchet, who ABSTAINED, said that the application lacked information that would have been necessary for an informed vote. He was surprised that polychaete specialists appear to have consistently treated *Malmgrenia* as a valid genus, and its originally only included species, *M. whiteavesii*, as a nomen dubium, or ‘virtually a nomen nudum’. Bouchet asked if *M. whiteavesii* is unrecognisable, how can *Malmgrenia* be recognisable? He would have liked to know how many biological species currently classified in *Malmgrenia* are known to occur in the Gulf of St. Lawrence, the type locality of *M. whiteavesii*. Obviously there can not be too many, as the World Register of Marine Species (WoRMS) lists just six valid species of *Malmgrenia* worldwide (Fauchald & Barnich, 2009). An obvious technical solution to stabilising *Malmgrenia* would have been to designate a neotype of *M. whiteavesii* from the type locality. Bouchet asked why this solution was not discussed.

Additional references

Fauchald, K. & Barnich, R. 2009. *Malmgrenia* McIntosh, 1874. In Fauchald, K. (Ed.), *World Polychaeta database* (Accessed through the World Register of Marine Species at <http://marinespecies.org/aphia.php?p=taxdetails&id=147006> on 2009-05-31).

Original references

The following are the original references to the names placed on Official Lists by the ruling given in the present Opinion:

andreapolis, McIntosh, 1874, *Annals and Magazine of Natural History*, (4)14: 195.
Malmgrenia McIntosh, 1874, *Annals and Magazine of Natural History*, (4)13: 263.

OPINION 2234 (Case 3416)***Murex rostratus* Olivi, 1792 (currently *Fusinus rostratus*; Mollusca, Gastropoda): specific name conserved**

Abstract. The Commission has conserved the specific name *Murex rostratus* Olivi, 1792 (currently *Fusinus rostratus*) (Mollusca, Gastropoda, FASCIOLARIIDAE) by ruling that it is not invalid by reason of being a junior primary homonym of *Murex rostratus* Solander in Brander, 1766 (currently *Turricula rostrata*). The senior homonym is currently included in TURRIDAE and has not been considered congeneric with *Murex rostratus* Olivi, 1792 after 1899.

Keywords. Nomenclature; taxonomy; Mollusca; Gastropoda; FASCIOLARIIDAE; TURRIDAE; *Fusinus rostratus*; *Turricula rostrata*; gastropods; Mediterranean.

Ruling

- (1) Under the plenary power it is hereby ruled that the name *rostratus* Olivi, 1792, as published in the binomen *Murex rostratus*, is not invalid by reason of being a junior primary homonym of *rostratus* Solander in Brander, 1766, as published in the binomen *Murex rostratus*.
- (2) The following names are hereby placed on the Official List of Specific Names in Zoology:
 - (a) *rostratus* Olivi, 1792, as published in the binomen *Murex rostratus* (FASCIOLARIIDAE), with the endorsement that it is not invalid by reason of being a junior primary homonym of *rostratus* Solander in Brander, 1766, as published in the binomen *Murex rostratus*;
 - (b) *rostratus* Solander in Brander, 1766, as published in the binomen *Murex rostratus* (TURRIDAE).

History of Case 3416

An application to conserve the specific name *Murex rostratus* Olivi, 1792 (currently *Fusinus rostratus*) (Mollusca, Gastropoda, FASCIOLARIIDAE) for a Mediterranean gastropod was received from Giovanni Buzzurro (*Biassono (MI), Italy*) and Paolo Russo (*Venezia, Italy*) on 18 April 2007. After correspondence the case was published in BZN 65: 7–11 (March 2008). The title, abstract and keywords of the case were published on the Commission's website. No comments were received on this case.

Decision of the Commission

On 1 March 2009 the members of the Commission were invited to vote on the proposals published in BZN 65: 8. At the close of the voting period on 1 June 2009 the votes were as follows:

Affirmative votes – 15: Bouchet, Brothers, Fautin, Halliday, Kullander, Krell, Lamas, Minelli, Ng, Papp, Patterson, Rosenberg, Štys, van Tol and Zhang.

Negative votes – 6: Alonso-Zarazaga, Bogutskaya, Grygier, Kottelat, Lim and Pape.

Pyle was on leave of absence.

Bouchet, voting FOR, said that the homonymy between *Murex rostratus* Solander in Brander, 1766 and *Murex rostratus* Olivi, 1792 was first detected by Bouchet & Warén (1985, p. 160), who stated ‘The next available name seems to be *Murex sanctaeluciae* von Salis, 1793, but a change of name should not be done until the shallow-water species are carefully revised, to avoid unnecessary name changes’. Buzzurro & Russo have adequately demonstrated that *Fusinus rostratus* remains the name overwhelmingly in use, and Bouchet has also kept using this name in popular field guides (Bouchet & Cosel 2001, 2009). He also added that the species has now been introduced in the English Channel, and there is a growing literature on marine invasive species (Le Duff et al., 2008; Nolf, 2009) that uses the name *Fusinus rostratus*. Bouchet therefore voted unreservedly for the conservation of the name *Murex rostratus* Olivi, 1792. Ng, also voting FOR, said that the case should have been brought much earlier for action, as the uncertainty over the name has been magnified in the interim from 1992 (when the junior primary homonym *Murex rostratus* Olivi, 1792 was replaced with its first available synonym by Aartsen & Giannuzzi-Savelli) until the present. He noted that this is the same principle as in Article 23.9.5, which allows for exceptions to primary homonymy when taxa are now classified in very different genera and families.

Voting AGAINST, Alonso-Zarazaga said that the authors were using the 4th Edition of the Code to amend decisions taken by correctly using the 3rd Edition of the Code, in force in 1992, when the junior primary homonym *Murex rostratus* Olivi, 1792 was replaced with its first available synonym, following exactly the relevant articles of the Code. He wondered why, again, 17 years later, someone was trying to keep a name that the application of the Code overrode. Moreover, Alonso-Zarazaga mentioned that this application had not raised a single comment, which he considered another indication of the negligible importance of the names treated in this case. Also voting AGAINST, Grygier pointed out that in paragraph 3 of the case, no citation is given for the first removal of one of the homonymous species from *Murex* before 1900. Without such explicit documentation of the two species’ supposedly long-standing non-congenerity, he felt it was hard to proceed further. More importantly, he said that this proposal depended on the inappropriate retroactive application of Article 23.9.5 to a situation arising from Aartsen & Giannuzzi-Savelli’s earlier correct application of Article 23(e) of the 3rd Edition of the Code, which was in force in 1992. Grygier also said that the authors did not discover the homonymy in question (it had already been noted and corrected), and so had no mandate under Article 23.9.5 to ask the Commission to consider the matter anew, although they were free to do so under some other provision of the Code. Grygier felt that the solution proposed by Aartsen & Giannuzzi-Savelli (1992) was formally correct at the time and should be upheld for the time being.

Additional references

Bouchet, P. & von Cosel, R. 2001. *Les coquillages des côtes françaises*. 32 pp. Editions Ouest France, Rennes. (Second edition, 2009).

- Bouchet, P. & Warén, A.** 1985. Revision of the Northeast Atlantic bathyal and abyssal Neogastropoda, excluding Turridae. *Bollettino Malacologico*, **suppl. 1**: 121–296.
- Le Duff, M., Grall, J. & Quiniou, L.** 2008. First record of the gastropod *Fusinus rostratus* (Mollusca: Fasciolaridae) on the northern coast of Brittany (western Channel, France). *JMBA2 Biodiversity Records*. 6236 Published online <http://www.mba.ac.uk/jmba/pdf/6236.pdf>
- Nolf, F.** 2009. Confirmation of the presence of *Fusinus rostratus* (Olivi, 1792) as a new invader in northern waters of Brittany (France). *Neptunea*, **8**(2): 9–16.

Original references

The following are the original references to the names placed on Official Lists by the ruling given in the present Opinion:

- rostratus*, *Murex*, Solander in Brander, 1766, Solander, D.C. 1766. Descriptions of species. In Brander, G. *Fossilia Hantoniensia collecta, et in Musaeo Brittanico deposita, a Gustavo Brander*. vi, Londini, p. 21.
- rostratus*, *Murex*, Olivi, 1792, *Zoologia Adriatica ossia Catalogo ragionato degli animali del golfo e delle lagune di Venezia; preceduto da una dissertazione sulla storia fisica e naturale del golfo; e accompagnato da memorie, ed osservazioni di fisica storia naturale ed economia*. x, 334, Bassano, p. 153.

OPINION 2235 (Case 3434)***Scleropauropus* Silvestri, 1902 (Myriapoda, Pauropoda): usage conserved**

Abstract. The Commission has conserved the usage of the name *Scleropauropus* Silvestri, 1902 for a group of pauropods (Myriapoda, Pauropoda) while also conserving as its type species the nominal species *S. hastifer* Silvestri, 1902, by replacing with an identical neotype the unsuitable holotypes of both *S. hastifer* Silvestri, 1902 (type species by monotypy) and *S. lyrifer* Remy, 1936 (consistently, but erroneously treated as the type species of *Scleropauropus* since Remy (1957)).

Keywords. Nomenclature; taxonomy; Myriapoda; Pauropoda; *Scleropauropus*; *Scleropauropus hastifer*; *Scleropauropus lyrifer*; pauropods; Europe.

Ruling

- (1) Under the plenary power it is hereby ruled that the holotypes of *Scleropauropus hastifer* Silvestri, 1902 and *Scleropauropus lyrifer* Remy, 1936 are set aside and the specimen at the Muséum National d'Histoire Naturelle, Paris, labelled 'Scleropauropus lyrifer Remy ad. Montgaillard près Périgueux. Été 1946. F. Grandjean' is designated as neotype of both species.
- (2) The name *Scleropauropus* Silvestri, 1902 (gender: masculine), type species by monotypy *Scleropauropus hastifer* Silvestri, 1902 is hereby placed on the Official List of Generic Names in Zoology.
- (3) The name *hastifer* Silvestri, 1902, as published in the binomen *Scleropauropus hastifer* (specific name of the type species of *Scleropauropus* Silvestri, 1902) and as defined by the neotype designated in (1) above, is hereby placed on the Official List of Specific Names in Zoology.
- (4) The name *lyrifer* Remy, 1936, as published in the binomen *Scleropauropus lyrifer* (a junior objective synonym of *Scleropauropus hastifer* Silvestri, 1902, as defined by the neotype designated in (1) above) is hereby placed on the Official Index of Rejected and Invalid Specific Names in Zoology.

History of Case 3434

An application to conserve the usage of the name *Scleropauropus* Silvestri, 1902 for a group of pauropods (Myriapoda, Pauropoda) while also conserving as its type species the nominal species *S. hastifer* Silvestri, 1902, by replacing with an identical neotype the unsuitable holotypes of both *S. hastifer* Silvestri, 1902 (type species by monotypy), and *S. lyrifer* Remy, 1936 (consistently, but erroneously treated as the type species of *Scleropauropus* since Remy (1957)) was received from Ulf Scheller (Järpås, Sweden) and Alessandro Minelli (University of Padova, Padova, Italy) on 26 July 2007. After correspondence the case was published in BZN 65: 17–19 (March 2008). The title, abstract and keywords of the case were published on the Commission's website. No comments were received on this case.

Decision of the Commission

On 1 March 2009 the members of the Commission were invited to vote on the proposals published in BZN 65: 19. At the close of the voting period on 1 June 2009 the votes were as follows:

Affirmative votes – 20: Alonso-Zarazaga, Bogutskaya, Bouchet, Brothers, Fautin, Halliday, Kottelat, Kullander, Krell, Lamas, Lim, Minelli, Ng, Pape, Papp, Patterson, Rosenberg, Štys, van Tol and Zhang.

Negative votes – 1: Grygier.

Pyle was on leave of absence.

Voting FOR the proposals, Ng stated that, as he is a proponent of simultaneous neotypes to solve intractable problems, this case had his full support.

Voting AGAINST the proposals, Grygier commented that to solve the main problem at hand (i.e. unambiguous characterisation of the genus), a neotype was only needed for *S. hastifer*. By focusing more on *S. lyrifer*, the proposed solution sought elegance at the expense of the spirit of Article 75.3.5 (likely conspecificity or not of the neotype with the holotype of *S. hastifer* was of lesser concern in this Application than its conspecificity with the holotype of *S. lyrifer*) and Article 75.3.6 (the proposed neotype was collected far from the type localities of both nominal species). Also, the authors cite no work accepting Remy's (1957) invalid proposal of *S. lyrifer* as type species, so their claim of recent consistent treatment in that respect is undocumented. A neotype candidate for *S. hastifer*, to propose to the Commission for approval, should be collected from around Rome; it can be a specimen displaying features that match the current *lyrifer*-based diagnosis of the genus, as long as it also matches the original description and what can be made of the present holotype.

Original references

The following are the original references to the names placed on Official Lists by the ruling given in the present Opinion:

hastifer, *Scleropauropus*, Silvestri, 1902 in Berlese, A. (Ed.), *Acari, Myriopoda et Scorpiones hucusque in Italia reperta*, vol. 10. Typ. 'Vesuviana', Portici. P. 66, pl. 13.

lyrifer, *Scleropauropus*, Remy, 1936, *Zoologischer Anzeiger*, **116**: 316.

Scleropauropus Silvestri, 1902 in Berlese, A. (Ed.), *Acari, Myriopoda et Scorpiones hucusque in Italia reperta*, vol. 10. Typ. 'Vesuviana', Portici. P. 66.

OPINION 2236 (Case 3424)**PSEUDOCOCCINI Cockerell, 1905 (Insecta, Hemiptera): precedence given over SPHAEROCOCCINI Cockerell, 1899**

Abstract. The Commission has conserved the usage of the family-group name PSEUDOCOCCINI Cockerell, 1905 (type genus *Pseudococcus* Westwood, 1840) for the mealybugs, a speciose group including many pests of agriculture, by giving it precedence over the senior family-group name SPHAEROCOCCINI Cockerell, 1899 (type genus *Sphaerococcus* Maskell, 1892).

Keywords. Nomenclature; taxonomy; Hemiptera; COCCOIDEA; PSEUDOCOCCINI; SPHAEROCOCCINI; *Pseudococcus*; *Sphaerococcus*; mealybugs.

Ruling

- (1) Under the plenary power it is hereby ruled that that the family-group name PSEUDOCOCCINI Cockerell, 1905 and other family-group names based on *Pseudococcus* Westwood, 1840 are to be given precedence over SPHAEROCOCCINI Cockerell, 1899 and other family-group names based on *Sphaerococcus* Maskell, 1892 whenever their type genera are placed in the same family-group taxon.
- (2) The name *Sphaerococcus* Maskell, 1892 (gender: masculine), type species by monotypy *Sphaerococcus casuarinae* Maskell, 1892 is hereby placed on the Official List of Generic Names in Zoology.
- (3) The following names are hereby placed on the Official List of Family-Group Names in Zoology:
 - (a) PSEUDOCOCCINI Cockerell, 1905 (type genus *Pseudococcus* Westwood, 1840), with the endorsement that it and other family-group names based on *Pseudococcus* are to be given precedence over SPHAEROCOCCINI Cockerell, 1899 and other family-group names based on *Sphaerococcus* Maskell, 1892 whenever their type genera are placed in the same family-group taxon;
 - (b) SPHAEROCOCCINI Cockerell, 1899 (type genus *Sphaerococcus* Maskell, 1892), with the endorsement that it and other family-group names based on *Sphaerococcus* are not to be given priority over PSEUDOCOCCINI Cockerell, 1905 and other family-group names based on *Pseudococcus* Westwood, 1840 whenever their type genera are placed in the same family-group taxon.

History of Case 3424

An application to conserve the usage of the family-group name PSEUDOCOCCINI Cockerell, 1905 (type genus *Pseudococcus* Westwood, 1840) for the mealybugs, a speciose group including many pests of agriculture, by giving it precedence over the senior family-group name SPHAEROCOCCINI Cockerell, 1899 (type genus *Sphaerococcus* Maskell, 1892) was received from Nate B. Hardy and Penny J. Gullan (*University of California, Davis, CA, U.S.A.*) on 5 June 2007. After correspondence the case was published in BZN 65: 24–26 (March 2008). The title, abstract and keywords of the case were published on the Commission's website. No comments were received on this case.

Decision of the Commission

On 1 March 2009 the members of the Commission were invited to vote on the proposals published in BZN 65: 25. At the close of the voting period on 1 June 2009 the votes were as follows:

Affirmative votes – 18: Alonso-Zarazaga, Bogutskaya, Brothers, Fautin, Grygier, Halliday, Kottelat, Kullander, Krell, Lamas, Minelli, Ng, Pape, Papp, Rosenberg, Štys, van Tol and Zhang.

Negative votes – 3: Bouchet, Lim and Patterson.

Pyle was on leave of absence.

Kottelat said he voted FOR the proposals only because this concerns a number of pests for agriculture and usage is demonstrated in the relevant literature and other documents. Ng also voted FOR, stating that, although the terms in this Case do contravene the Principle of Priority, he noted that the family name PSEUDOCOCCIDAE has been placed on the Official Lists, is a family of some commercial significance, and a name change here is likely to have an impact. Much as he disliked breaking the Principle of Priority, he felt there is reasonable basis to concur with the applicants.

Bouchet voted AGAINST the proposals, explaining that these conditional reversals of precedence are a source of nomenclatural instability.

Original references

The following are the original references to the names placed on Official Lists by the ruling given in the present Opinion:

PSEUDOCOCCINI Cockerell, 1905, *Colorado University Studies*, 2: 193.

SPHAEROCOCCINI Cockerell, 1899, *Bulletin of the Illinois State Laboratory of Natural History*, 5: 389.

Sphaerococcus Maskell, 1892, *Transactions and Proceedings of the New Zealand Institute*, 24: 39.

OPINION 2237 (Case 3422)***Helops* Fabricius, 1775 (Insecta, Coleoptera, TENEBRIONIDAE): usage conserved by designation of *Tenebrio caeruleus* Linnaeus, 1758 as the type species**

Abstract. The Commission has conserved the current usage of the widely used name *Helops* Fabricius, 1775 (tenebrionid beetle from the Western Palaearctic, Nearctic and Neotropical Regions) by designation of *Tenebrio caeruleus* Linnaeus, 1758 as the type species. All type fixations for the nominal genus *Helops* Fabricius, 1775 before that of *Tenebrio caeruleus* Linnaeus, 1758 by Hope (1840) have been set aside.

Keywords. Nomenclature; taxonomy; Coleoptera; TENEBRIONIDAE; HELOPINI; HELOPINA; CYLINDRINOTINA; *Helops*; *Helops lanipes*; *Tenebrio caeruleus*; darkling beetles; Western Palaearctic; Nearctic and Neotropical Regions.

Ruling

- (1) Under the plenary power it is hereby ruled that all type fixations for the nominal genus *Helops* Fabricius, 1775 before that of *Tenebrio caeruleus* Linnaeus, 1758 by Hope (1840) are set aside.
- (2) The name *Helops* Fabricius, 1775 (gender: masculine), type species *Tenebrio caeruleus* Linnaeus, 1758 by the fixation of Hope (1840) as ruled in (1) above is hereby placed on the Official List of Generic Names in Zoology.
- (3) The name *caeruleus* Linnaeus, 1758 as published in the binomen *Tenebrio caeruleus* (specific name of the type species of *Helops* Fabricius, 1775) is hereby placed on the Official List of Specific Names in Zoology.

History of Case 3422

An application to conserve the current usage of the widely used name *Helops* Fabricius, 1775 (tenebrionid beetle from the Western Palaearctic, Nearctic and Neotropical Regions) by designation of *Tenebrio caeruleus* Linnaeus, 1758 as the type species was received from Maxim Nabozhenko (*Southern Scientific Centre of the Russian Academy of Sciences, Rostov-on-Don, Russia*), Patrice Bouchard (*Canadian National Collection of Insects, Arachnids and Nematodes, Agriculture and Agri-Food Canada, Ottawa, Ontario, Canada*) and Ivan Löbl (*Muséum d'Histoire Naturelle, Geneva, Switzerland*) on 28 May 2007. After correspondence the case was published in BZN 65: 27–30 (March 2008). The title, abstract and keywords of the case were published on the Commission's website. No comments were received on this case.

Decision of the Commission

On 1 March 2009 the members of the Commission were invited to vote on the proposals published in BZN 65: 28. At the close of the voting period on 1 June 2009 the votes were as follows:

Affirmative votes – 18: Alonso-Zarazaga, Bouchet, Brothers, Fautin, Grygier, Halliday, Kottelat, Krell, Lamas, Lim, Minelli, Ng, Pape, Papp, Rosenberg, Štys, van Tol and Zhang.

Negative votes – 3: Bogutskaya, Kullander and Patterson.

Pyle was on leave of absence.

Voting FOR the proposals, Grygier said he would have liked to know the number of species in the genera and subtribes whose names were at stake. Ng also voted FOR, explaining that his personal preference is to keep to the original type species designation but he sympathised with the messiness described in this Case so he supported the proposals, as he felt this would cause the least confusion. He added that the way this mistake had been perpetuated since Latreille was most unfortunate.

Kullander voted AGAINST the proposals, stating that Iablokoff-Khnzorian (1964) obviously demonstrated which was the correct type species and that entomologists had had 45 years to adapt.

Original references

The following are the original references to the names placed on Official Lists by the ruling given in the present Opinion:

caeruleus, *Tenebrio*, Linnaeus, 1758, *Systema Naturae*, Ed. 10, vol. 1, p. 418.

Helops Fabricius, 1775, *Systema entomologiae, sistens insectorum classes, ordines, genera, species, adiectis synonymis, locis, descriptionibus, observationibus*. Xxxii, p. 257.

The following is the reference for the designation of *Tenebrio caeruleus* Linnaeus, 1758 as the type species of the nominal genus *Helops* Fabricius, 1775:

Hope, F.W. 1840. *The coleopterist's manual, part the third, containing various families, genera, and species, of beetles, recorded by Linneus and Fabricius. Also, descriptions of newly discovered and unpublished insects*. [5], p. 133.

OPINION 2238 (Case 3419)

**CORYSTIDAE Foster & Philip, 1978 (Echinodermata, Echinoidea):
spelling emended to CORYSTUSIDAE to remove homonymy with
CORYSTIDAE Samouelle, 1819 (Crustacea, Brachyura)**

Abstract. The Commission has ruled that the homonymy between the family-group names CORYSTIDAE Foster & Philip, 1978 (Echinodermata, Echinoidea) and CORYSTIDAE Samouelle, 1819 (Crustacea, Brachyura) be removed by adopting *Corystus*- as the stem of the name of the type genus *Corystus* Pomel, 1883, giving the corresponding echinoderm family-group name CORYSTUSIDAE.

Keywords. Nomenclature; taxonomy; CORYSTIDAE; CORYSTUSIDAE; *Corystus*; *Corystes cassivelaunus*; *Corystus dysasteroides*; Tertiary fossils; masked carb; Europe; Australia; New Zealand.

Ruling

- (1) Under the plenary power it is hereby ruled that the stem of the generic name *Corystus* Pomel, 1883 is *Corystus*-.
- (2) The entry on the Official List of Generic Names of Zoology for the name *Corystes* Bosc is hereby emended to record that the correct date of publication of Bosc's paper is prior to 28 April 1802.
- (3) The name *Corystus* Pomel, 1883 (gender: masculine), type species by original designation *Rhynchopygus dysasteroides* Duncan, 1877, is hereby placed on the Official List of Generic Names in Zoology.
- (4) The name *dysasteroides* Duncan, 1877, as published in the binomen *Rhynchopygus dysasteroides* (specific name of the type species of *Corystus* Pomel, 1883), is hereby placed on the Official List of Specific Names in Zoology.
- (5) The name CORYSTUSIDAE Foster & Philip, 1978, type genus *Corystus* Pomel, 1883 (spelling emended by the ruling in (1) above) (Echinodermata, Echinoidea), is hereby placed on the Official List of Family-Group Names in Zoology.
- (6) The name CORYSTIDAE Foster & Philip, 1978 is hereby placed on the Official Index of Rejected and Invalid Family-Group Names in Zoology.

History of Case 3419

An application to remove the homonymy between the Family-Group names CORYSTIDAE Foster & Philip, 1978 (Echinodermata, Echinoidea) and CORYSTIDAE Samouelle, 1819 (Crustacea, Brachyura) by emending the stem of the name of the type genus *Corystus* Pomel, 1883 to *Corystus*- to change the spelling of the echinoderm family-group name to CORYSTUSIDAE, was received from Christopher B. Boyko (*Molloy College, Rockville Centre, NY, U.S.A.*) on 18 April 2007. After correspondence the case was published in BZN 65: 114–118 (June 2008). The title, abstract and keywords of the case were published on the Commission's website. A supportive comment on this case was published in BZN 65: 309 (December 2008).

Decision of the Commission

On 1 June 2008 the members of the Commission were invited to vote on the proposals published in BZN **65**: 116 At the close of the voting period on 1 September 2009 the votes were as follows:

Affirmative – 26: Alonso-Zarazaga, Bogutskaya, Bouchet, Brothers, Fautin, Grygier, Halliday, Harvey, Kojima, Kottelat, Krell, Kullander, Lamas, Lim, Minelli, Ng, Pape, Papp, Patterson, Rosenberg, Stys, Tol, Winston, Yanega, Zhang and Zhou,

Negative – 1: Ballerio.

Pyle was on leave of absence.

Original references

The following are the original references to the names placed on Official Lists and Indexes by the ruling given in the present Opinion:

CORYSTIDAE Foster & Philip, 1978, *Palaeontology*, **21**(4): 792.

CORYSTUSIDAE Foster & Philip, 1978, *Palaeontology*, **21**(4): 792

Corystus Pomel, 1883, *Classification méthodique et genera des échinides vivants & fossiles*, p. 61.
dysasteroides, *Rhynchopygus*, Duncan, 1877, *Quarterly Journal of the Geological Society of London*, **33**: 49.

OPINION 2239 (Case 3383)***Gobius lagocephalus* Pallas, 1770 (currently *Sicyopterus lagocephalus*; Osteichthyes, Teleostei, GOBIIDAE): specific name not suppressed**

Abstract. The Commission did not approve a proposal to suppress the name *Gobius lagocephalus* Pallas, 1770 (currently *Sicyopterus lagocephalus*), commonly used for a species of sicydiine goby.

Keywords. Nomenclature; taxonomy; Teleostei; GOBIIDAE; SICYDIINAE; *Gobius*; *Sicyopterus lagocephalus*; western Atlantic; Indo-West Pacific; goby.

Ruling

- (1) A proposal to suppress the name *Gobius lagocephalus* Pallas, 1770 (currently *Sicyopterus lagocephalus*), was not approved.
- (2) The name *lagocephalus* Pallas, 1770, as published in the binomen *Gobius lagocephalus* Pallas, 1770 and as designated by the neotype (Forschungsinstitut und Naturmuseum Senckenberg, Frankfurt am Main, Germany, SMF 28571) with type locality Ravine St. Gilles on Réunion Island, is hereby placed on the Official List of Specific Names in Zoology.

History of Case 3383

An application to suppress the name *Gobius lagocephalus* Pallas, 1770 (currently *Sicyopterus lagocephalus*), commonly used for a species of sicydiine goby, was received from Wm. Leo Smith and John S. Sparks (*American Museum of Natural History, New York, NY 10024, U.S.A*) on 24 May 2006. After correspondence the case was published in BZN 64: 103–107 (June 2007). The title, abstract and keywords of the case were published on the Commission's website. A comment by Kottelat et al. opposing this case and presenting two alternative proposals was published in BZN 65: 57–60. Six additional comments opposing this case were published in BZN 65: 150–151; 309–310.

In their comment, Kottelat et al. (2008) noted that the issue had been recently addressed by Kottelat (2007) and solved by the designation of a neotype, and that a ruling by the Commission was not necessary. Kottelat et al. (2008) proposed that the designation be confirmed by the Commission. This is not necessary, because Kottelat (2007) provided a valid designation under the Code.

Decision of the Commission

On 1 June 2009 the members of the Commission were invited to vote on the proposals published in BZN 64: 106. At the close of the voting period on 1 September 2009 the votes were as follows:

Affirmative votes – 1: Zhou.

Negative votes – 26: Alonso-Zarazaga, Ballerio, Bogutskaya, Bouchet, Brothers, Fautin, Grygier, Halliday, Harvey, Kojima, Kottelat, Krell, Kullander, Lamas, Lim,

Minelli, Ng, Pape, Papp, Patterson, Rosenberg, Stys, Tol, Winston, Yanega and Zhang.

Pyle was on leave of absence.

Fautin, voting AGAINST, said that the comments, which were all against the proposal, made a persuasive case that the situation was now stable, and instability would be induced by suppressing the name. Halliday explained that he voted AGAINST because the matter had already been resolved, in the paper by Kottelat (2007). Kojima, voting AGAINST, agreed that the application was not acceptable for the reasons clearly stated by Kottelat et al. (2008). Similarly, voting AGAINST, Kullander said this was an unnecessary case, and Kottelat et al. (2008) should be followed. Stys voted AGAINST and said he fully agreed with the comment provided by Kottelat et al. (2008). Winston voted AGAINST, and said it is a nomen dubium, but the arguments of both sides seemed to her to be as much political as scientific. Since the most clamorous users are in the Pacific, she said it would seem best for one of them to correctly designate a neotype, if they wanted to keep the name. Ng voted AGAINST, although he agreed with the applicants that there was a big problem. However, he disagreed with the proposed solution of suppressing this name, especially in view of the fact that the name was now in widespread use, and that the taxonomy of this species and its synonyms, some of which may be good species, was still far from settled. He felt that a more rational approach would be to select a suitable neotype that would best keep how the name was in broad use today. It may conflict against the original intent, or purposes, but this was far less important than keeping current usage stable. He also noted that there was no evidence that the purported type locality was correct or even if the original datasets cited were accurate.

Original references

The following is the original reference to the name placed on the Official List by the ruling given in the present Opinion:

lagocephalus, *Gobius*, Pallas, 1770, *Spicilegia Zoologica quibus novae imprimis et obscurae animalium species iconibus, descriptionibus atque commentaries illustrantur*, vol. 1, fasc. 8. Gottl. August. Lange, Berolini, p. 14.

The following is the reference for the designation of the neotype for *Gobius lagocephalus* Pallas, 1770:

Kottelat, M. 2007. *Molecular Phylogenetics and Evolution*, **43**: 693–695.

OPINION 2240 (Case 3418)**Specific names of nine Australian birds (Aves) conserved**

Abstract. The Commission has conserved the usage of nine specific names of common Australian birds. The names were chosen as senior by First Revisers, but were later replaced almost a hundred years ago by simultaneously published names on grounds of page precedence. They have now been stabilised.

Keywords. Nomenclature; taxonomy; *Anthochaera chrysoptera*; *Geopelia placida*; *Geopelia tranquilla*; *Gerygone olivacea*; *Grallina cyanoleuca*; *Leucosarcia melanoleuca*; *Lichenostomus melanops*; *Manorina melanocephala*; *Oriolus sagittatus*; *Psophodes olivaceus*; little wattlebird; peaceful dove; white-throated gerygone; magpielark; wonga pigeon; yellow-tufted honeyeater; noisy miner; olive-backed oriole; eastern whipbird; Australia.

Ruling

(1) Under the plenary power it is hereby ruled:

(a) that the following specific names are suppressed for the Purposes of the Principle of Priority but not for those of the Principle of Homonymy:

- (i) *albogularis* Gould, 1838, as published in the binomen *Psilopus albogularis*;
- (ii) *auricomis* Latham, 1801, as published in the binomen *Muscicapa auricomis*;
- (iii) *crepitans* Latham, 1801, as published in the binomen *Muscicapa crepitans*;
- (iv) *garrulus* Latham, 1801, as published in the binomen *Merops garrulus*;
- (v) *mellivora* Latham, 1801, as published in the binomen *Certhia mellivora*;
- (vi) *picata* Latham, 1801, as published in the binomen *Columba picata*;
- (vii) *picata* Latham, 1801, as published in the binomen *Gracula picata*;
- (viii) *viridis* Latham, 1801, as published in the binomen *Gracula viridis*;

(b) that the name *placida* Gould, 1844, as published in the binomen *Geopelia placida*, is to be given precedence over the name *tranquilla* Gould, 1844, as published in the binomen *Geopelia tranquilla*, whenever the two are considered to be synonyms.

(2) The following names are hereby placed on the Official List of Specific Names in Zoology:

- (a) *chrysopterus* Latham, 1801, as published in the binomen *Merops chrysopterus*;
- (b) *cyanoleucus* Latham, 1801, as published in the binomen *Corvus cyanoleucus*;
- (c) *melanocephala* Latham, 1801, as published in the binomen *Gracula melanocephala*;

- (d) *melanoleuca* Latham, 1801, as published in the binomen *Columba melanoleuca*;
 - (e) *melanops* Latham, 1801, as published in the binomen *Turdus melanops*;
 - (f) *olivaceus* Latham, 1801, as published in the binomen *Corvus olivaceus*;
 - (g) *olivaceus* Gould, 1838, as published in the binomen *Psilopus olivaceus*;
 - (h) *placida* Gould, 1844, as published in the binomen *Geopelia placida*, with the endorsement that it is to be given precedence over the name *tranquilla* Gould, 1844, as published in the binomen *Geopelia tranquilla*, whenever the two are considered to be synonyms;
 - (i) *sagittata* Latham, 1801, as published in the binomen *Coracias sagittata*;
 - (j) *tranquilla* Gould, 1844, as published in the binomen *Geopelia tranquilla*, with the endorsement that it is not to be given priority over the name *placida* Gould, 1844, as published in the binomen *Geopelia placida* whenever the two are considered to be synonyms.
- (3) The following names are hereby placed on the Official Index of Rejected and Invalid Specific Names in Zoology:
- (a) *albogularis* Gould, 1838, as published in the binomen *Psilopus albogularis* and as suppressed in 1(a) above;
 - (b) *auricomis* Latham, 1801, as published in the binomen *Muscicapa auricomis*, and as suppressed in 1(b) above;
 - (c) *crepitans* Latham, 1801, as published in the binomen *Muscicapa crepitans*, and as suppressed in 1(c) above;
 - (d) *garrulus* Latham, 1801, as published in the binomen *Merops garrulus*, and as suppressed in 1(d) above;
 - (e) *mellivora* Latham, 1801, as published in the binomen *Certhia mellivora*, and as suppressed in 1(e) above.
 - (f) *picata* Latham, 1801, as published in the binomen *Columba picata* and as suppressed in 1(f) above;
 - (g) *picata* Latham, 1801, as published in the binomen *Gracula picata* and as suppressed in 1(g) above;
 - (h) *viridis* Latham, 1801, as published in the binomen *Gracula viridis*, and as suppressed in 1(h) above;

History of Case 3418

An application to conserve the usage of nine specific names of common Australian birds by suppressing names chosen by First Revisers was received from Richard Schodde (*Australian Biological Resources Study, Canberra, Australia*) and Walter J. Bock (*Columbia University, New York, NY, U.S.A.*) on 19 April 2007. After correspondence the case was published in BZN **65**: 35–41 (March 2008). The title, abstract and keywords of the case were published on the Commission's website. No comments were received on this case.

Decision of the Commission

On 1 March 2009 the members of the Commission were invited to vote on the proposals published in BZN **65**: 38–39. At the close of the voting period on 1 June 2009 the votes were as follows:

Affirmative votes – 17: Bouchet, Brothers, Fautin, Grygier, Halliday, Kottelat, Krell, Lamas, Minelli, Ng, Pape, Papp, Patterson, Rosenberg, Štys, van Tol and Zhang.

Negative votes – 3: Bogutskaya, Kullander and Lim.

Alonso-Zarazaga split his vote, voting affirmative for proposals (2) and (3) and negative for proposals (1) and (4). Pyle was on leave of absence.

Kottelat explained that he voted FOR the proposals only in the hope of closing the case. He would have preferred it if these nine names had not been suppressed, but instead placed on the Official List, with mention that for each name the simultaneous synonym should be given precedence whenever the two are considered to be synonyms. Ng, also voting FOR, said he found it instructive that seemingly the preponderance of ornithologists has continued using the names as selected by line precedence rather than First Reviser principle. The latter rule was originally established to allow revisers to choose a name that best maintains stability, even if line precedence is not followed. This being the intent of the rule, he was therefore obliged to agree with the applicants that stability was best maintained through keeping the names as currently used.

Alonso-Zarazaga, SPLITTING his vote, said it was undesirable to suppress eight specific names of birds that may be needed in the future, since they are subjective synonyms. He was against this waste of names for sibling or sub-species uncovered by using more sensitive techniques such as DNA sequencing. Alonso-Zarazaga advocated that, in general, when the Commission screens submitted applications, it should discourage authors from applying for plain suppressions. He suggested that reversals of precedence achieve stability and are a more elegant nomenclatural solution.

Original references

The following are the original references to the names placed on Official Lists by the ruling given in the present Opinion:

- albogularis*, *Psilopus*, Gould, 1838, *A synopsis of the birds of Australia and the adjacent islands*, Part 4, pl. 61.
- auricomis*, *Muscicapa*, Latham, 1801, *Supplementum Indicis Ornithologici, sive Systematis Ornithologiae*, p. 49.
- chrysopterus*, *Merops*, Latham, 1801, *Supplementum Indicis Ornithologici, sive Systematis Ornithologiae*, p. 33.
- crepitans*, *Muscicapa*, Latham, 1801, *Supplementum Indicis Ornithologici, sive Systematis Ornithologiae*, p. 51.
- cyanoleucus*, *Corvus*, Latham, 1801, *Supplementum Indicis Ornithologici, sive Systematis Ornithologiae*, p. 25.
- garrulous*, *Merops*, Latham, 1801, *Supplementum Indicis Ornithologici, sive Systematis Ornithologiae*, p. 34.
- melanocephala*, *Gracula*, Latham, 1801, *Supplementum Indicis Ornithologici, sive Systematis Ornithologiae*, p. 28.
- melanoleuca*, *Columba*, Latham, 1801, *Supplementum Indicis Ornithologici, sive Systematis Ornithologiae*, p. 59.
- melanops*, *Turdus*, Latham, 1801, *Supplementum Indicis Ornithologici, sive Systematis Ornithologiae*, p. 40.
- mellivora*, *Certhia*, Latham, 1801, *Supplementum Indicis Ornithologici, sive Systematis Ornithologiae*, p. 37.

- olivaceus*, *Corvus*, Latham, 1801, *Supplementum Indicis Ornithologici, sive Systematis Ornithologiae*, p. 26.
- olivaceus*, *Psilopus*, Gould, 1838, *A synopsis of the birds of Australia and the adjacent islands*, Part 4, pl. 61.
- picata*, *Columba*, Latham, 1801, *Supplementum Indicis Ornithologici, sive Systematis Ornithologiae*, p. 59.
- picata*, *Gracula*, Latham, 1801, *Supplementum Indicis Ornithologici, sive Systematis Ornithologiae*, p. 29.
- placida*, *Geopelia*, Gould, 1844, *Proceedings of the Zoological Society of London*, **12**: 55.
- sagittata*, *Coracias*, Latham, 1801, *Supplementum Indicis Ornithologici, sive Systematis Ornithologiae*, p. 26.
- tranquilla*, *Geopelia*, Gould, 1844, *Proceedings of the Zoological Society of London*, **12**: 56.
- viridis*, *Gracula*, Latham, 1801, *Supplementum Indicis Ornithologici, sive Systematis Ornithologiae*, p. 28.

Notice of closure of Cases

The following Cases, for which receipts as new applications to the Commission were published though the cases were never published in full, are now closed:

Hornera frondiculata Lamouroux, 1821 (Bryozoa, Cyclostomata): proposed conservation. A. Smith, P.D. Taylor & H.G. Spencer (Case 3448: acknowledgement of receipt published in BZN 65: 2).

Libellula isoceles Müller, 1767 (currently *Aeshna isoceles*; Odonata, Anisoptera, AESHNIDAE); proposed emendation of spelling to *Libellula isosceles*. P.J. Mill, S. Brooks, N. Moore & P. Taylor) (Case 3465: acknowledgement of receipt published in BZN 65: 82). The application has been withdrawn by the authors.

Neofelis Gray, 1867 (Mammalia, PANTHERIDAE): re-evaluation of specific names. P. Christiansen (Case 3457: acknowledgement of receipt published in BZN 65: 82).

Rhynchotherium tascalae Osborn, 1918: proposed designation of a neotype. S.G. Lucas (Case 3478: acknowledgement of receipt published in BZN 65: 242).

Massospondylus carinatus Owen, 1854 (Dinosauria, Sauropodomorpha): proposed conservation of usage by designation of a neotype. A.M. Yates & P.M. Barrett (Case 3478: acknowledgement of receipt published in BZN 66: 203).

The published Case ‘*Conus jaspideus* Gmelin, 1791 (Mollusca, Gastropoda): proposed conservation of the specific name by designation of a neotype’ A.J. Kohn & D.L.N. Vink (Case 3396: acknowledgement of receipt published in BZN 63: 221; published in BZN 64: 144–148) has been closed without a vote. A comment by G. Rosenberg & M. Grygier published in BZN 65(3): 214 pointed out that Vink’s (1991) neotype designation is valid under the Code and therefore there is no need for action by the Commission.

Corrigendum to Case 3485. *Lychnorhiza lucerna* Haeckel, 1880 (Cnidaria, Scyphozoa, Rhizostomeae): proposed conservation of generic and specific names (BZN: 66(3): 242–246)

The last sentence of para. 14 on p. 244 should read: The name *cyanolobatus* Agassiz, 1862 has not been used as valid since 1899 (which meets the conditions of Article 23.9.1.1 of the Code), and the conditions of Article 23.9.1.2 are also met for *Lychnorhiza lucerna*; thus it is possible to apply Article 23.9.2 of the Code and declare the name *cyanolobatus* Agassiz, 1862 a nomen oblitum.

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**NAMES AND WORKS PLACED ON OFFICIAL LISTS AND INDEXES OR
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Names and Works placed on the Official Lists and Indexes in Volume 66 are listed below. Entries on the Official Lists are in bold type and those on the Official Indexes in non-bold type.

- albogularis*, *Psilopus*, Gould, 1838 (Aves). Op. 2240
andreapolis, ***Malmgrenia***, McIntosh, 1874 (Annelida). Op. 2233
angustula, ***Buprestis***, Illiger, 1803 (Coleoptera). Op. 2221
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auricomis, *Muscicapa*, Latham, 1801 (Aves). Op. 2240
- bicolor***, ***Phyllobates***, Bibron in Cocteau & Bibron, 1840 (Amphibia). Op. 2223
- caeruleus***, ***Tenebrio***, Linnaeus, 1758 (Coleoptera). Op. 2237
chrysopterus, ***Merops***, Latham, 1801 (Aves). Op. 2240
Corystes Bosc, 1802 (Crustacea). Op. 2238
CORYSTIDAE Foster & Philip, 1978 (Echinoidea). Op. 2238
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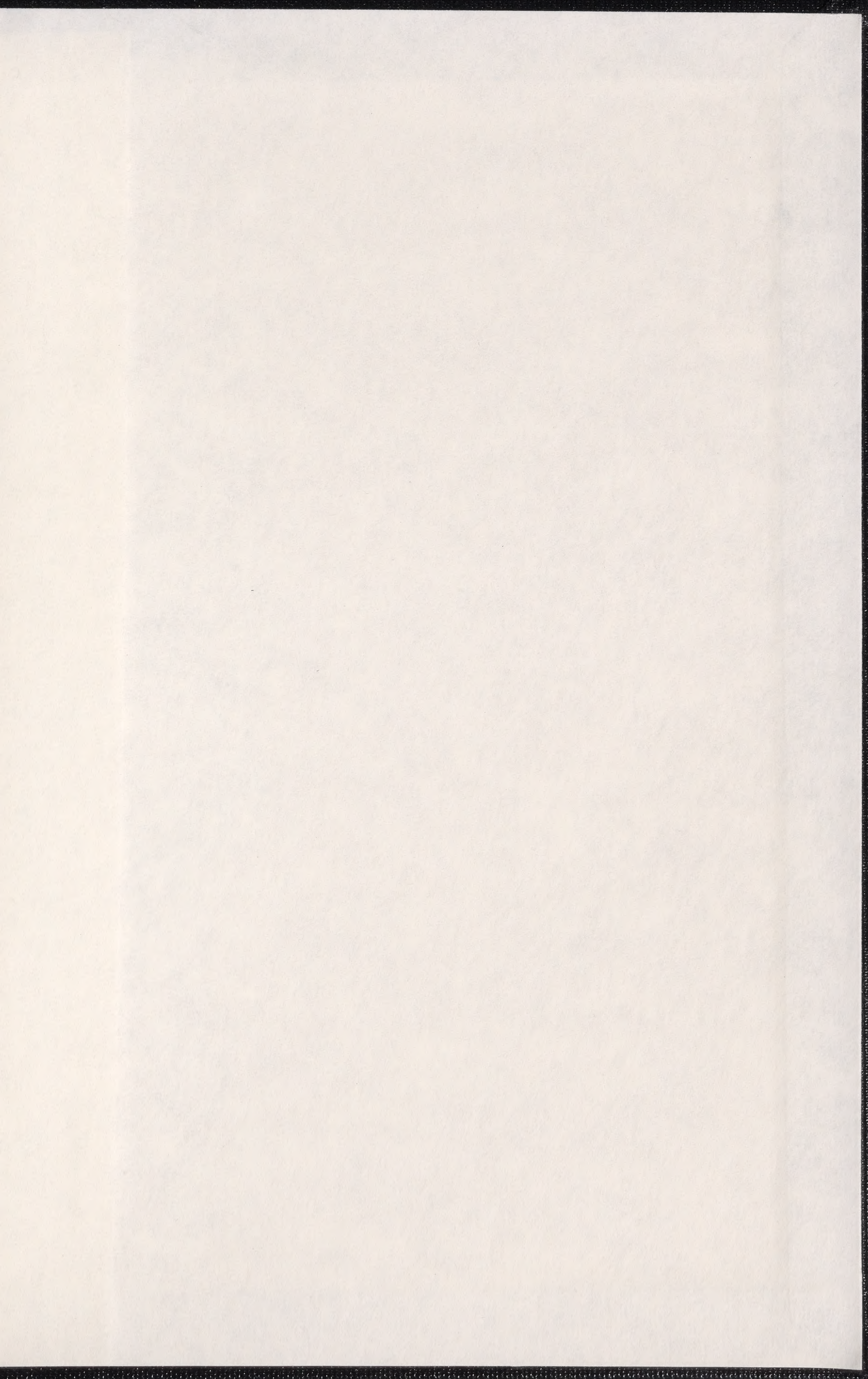
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